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THE PLANETESIMAL HYPOTHESIS[1]

ON the eighty-fifth anniversary of his birth,[1a] September 25, 1928, Professor Chamberlin's latest book appeared from the press. In this volume he presents an orderly account of the researches he has made during the last thirty years respecting the origin and development of the planets and the other attendants of the sun. Lest any one should suppose that this work represents the feeble and distorted echoes of more vigorous years, I wish to say at once that it would be difficult to find in astronomical literature its superior in exhaustiveness and coherence of reasoning, in precision of statement and in the exercise of constructive imagination. In his alertness for significant clues and interrelations among phenomena and in the relentlessness with which he pursues the trails on which he enters, he reminds one of Charles Darwin in "The Origin of Species."

On the same day that Professor Chamberlin's book was received, I found in the October *Harper's Magazine,* page 574, in an article by Professor Eddington, of Cambridge, England, the following sentences:

> By elimination of alternatives it appears that a configuration resembling the solar system would be formed only if at a certain stage of condensation an unusual accident had occurred. According to Jeans, the accident was the close approach of another star casually pursuing its way through space. This star must have passed within a distance not far outside the orbit of Neptune; it must not have passed too rapidly, but have slowly overtaken or been overtaken by the sun. By tidal distortion it raised big protuberances on the sun, and caused it to spurt out filaments of matter which have condensed to form planets. That was more than a thousand million years ago. The intruding star has since gone on its way and mingled with the others; its legacy of a system of planets remains, including a globe habitable by man.

These sentences are the most recent expression of wholly erroneous views respecting the authorship of the theory to which they refer. They are the culmina-

[1] *The Two Solar Families: The Sun's Children.* By Thomas Chrowder Chamberlin. Chicago, The University of Chicago Press, 1928. 8vo., pp. xxi + 311; 52 illustrations. $2.50.

[1a] The death of Professor Chamberlin occurred on November 15, after this review of his book had been finished.

tion of an increasing disregard for facts which are incontestably established. Irrespective of the question of property rights, the interests of science itself demand that silence no longer be maintained on this subject. Consequently, and particularly because Professor Chamberlin himself has not referred to it, I shall make, in later paragraphs, such a clear and explicit statement of the actual history of the initiation and of the development of the ideas respecting the origin of the planets from the dynamic effects of a passing star that there will not again be any excuse for such a statement as that quoted from Professor Eddington. But before this matter is taken up, an attempt will be made to indicate the general scope of Professor Chamberlin's book.

Professor Chamberlin divided his book into three major parts. The first part contains an account of his (and my) critical examination and abandonment of earlier theories of the origin of the planets, work which was essentially completed by the year 1900; the second consists of the development of the planetesimal hypothesis, including as an essential part the rôle played by a passing star at the birth of the planetary system, a work all of whose main outlines were laid down and published by 1906; and the third is devoted to more recent auxiliary developments in various directions, and in particular to his theories respecting the origin of meteors and comets.

At first one might question, as I did, the advisability of using eighty pages in proving earlier theories erroneous, for it is now almost twenty-nine years since he and I, in separate investigations,[2] examined the Laplacian theory and found it absolutely untenable. The tests to which it was submitted were so conclusive as to leave no room for its continued acceptance. But inherited ideas are tenaciously held and new ones are adopted very slowly. For example, in 1911, Professor Frost, writing in *Popular Astronomy* (9: 466–7), excellently expressed the views generally held by astronomers in the following words: "But no adequate substitute [for the Laplacian theory] has been proposed, and the increased study of the different phases of development, as inferred from stellar spectra, supports the Laplacian theory surprisingly." Even in 1922, Dr. George E. Hale, in his book, "The New Heavens" (page 35), said: "Laplace's hypothesis has been subjected in recent years to much criticism, and there is good reason to doubt whether his description of the mode of evolution of our solar system is correct in every particular." It is only within the last four or five years that astronomers have shown any general inclination to regard the ideas associated with the Laplacian theory as untenable. For these reasons, as well as for the fact that many books on astronomy and geology still fail to point out the grave defects of the Laplacian theory, Professor Chamberlin is probably quite right in completely clearing the ground of inherited ideas.

One reason that the fundamental ideas which are basic in the planetesimal hypothesis have not been readily grasped is that this hypothesis is not simply a variant of the nebular theory of Laplace. The Laplacian theory and the doctrines associated with it constitute one genus of scientific theories; the planetesimal and associated hypotheses constitute a wholly different genus. The gap between these different genera of intellectual constructions is as profound as that between different genera of living organisms, and as difficult to bridge.

In order to illustrate the revolution in fundamental points of view which the new hypotheses require, the principal theories of the Laplacian group will be briefly enumerated and contrasted with the planetesimal hypothesis. Laplace started with a heated gaseous mass rotating as a solid. With loss of heat by radiation, it contracted and rotated more rapidly. At various stages of the contraction, the centrifugal acceleration at the equator of the rotating mass equaled the gravitational acceleration toward its center. At these places the contracting mass left behind gaseous rings which were concentrated into planets by the mutual gravitation of their parts. In six cases, after the contracting rings had assumed approximately spherical forms they similarly contracted and left behind smaller rings, which became satellites. This theory is delightfully simple and can be stated in a few sentences. It makes few demands upon the imagination to conceive of its various steps, and it requires no sustained mental effort to organize them into a unified whole. It raises no unanswered questions and arouses no doubts. The account of the creation and the origin of the earth in Genesis is not simpler.

Associated with the Laplacian theory is the theory that the earth is fluid except for a relatively thin crust. The phrase "the crust of the earth" occurs in geology about as frequently as the words "air" or "water." Dynamic geology has had this concept as its foundation.

Helmholtz's contraction theory of the sun's heat is another member of the same group of hypotheses. It relieves the Laplacian theory of the necessity for an excessively hot original nebula, and it measures the life of the earth and the other planets. Consequently it assigns superior limits to the past duration of the geological processes and the evolutionary stages of

[2] *Journal of Geology*, 8 (1900): 58–73; *Astrophysical Journal*, 11 (1900): 103–130.

living organisms, and it foretells the time when they will have come to an end. The time-intervals are measured in tens of millions of years at the most.

Still another theory built upon the same ideas and harmonious with them, though insisting upon a variant in the case of the moon, is Sir George Darwin's splendidly elaborated tidal evolution. In thoroughness of examination and in clarity and fairness of exposition, Darwin's work is a model. But the body tides of the earth are now known not to be a dominant factor in the evolution of the earth-moon system, because the inherited ideas that the earth has a fluid or viscous interior have been found to be erroneous.

In the celestial spaces astronomers have held unquestioningly the same order of ideas. According to views current almost up to the present time, each star is going through a somewhat similar evolution. In some cases, the original mass undergoes fission into two comparable masses, a binary star; in others, presumably rings are left behind and planets are formed; in still others having very slow initial rotations, the nebulae concentrate into single stars unattended by planets. The evolution of each star, with very rare exceptions, is wholly independent of every other star. Observations are held to support the theory. Some stars have simple spectra—they are the stars recently concentrated from nebulae. Astronomical literature is filled with references to "young stars" and "early stars," and this simple theory of stellar evolution has dominated astronomical thought. When Professor Campbell finds that stars of "early" spectral types move much more slowly than the others, he questions whether the explanation of the fact may not be that they had not been long enough in the star state for gravitation to produce large relative velocities ("Stellar Motions" [1913], p. 216). In interpreting the giant and the dwarf stars in connection with their spectral types, Professor Russell follows out the contraction theory of stellar heat more consistently. He has the stars contracting from nebulae, and in the process running up in temperature to a maximum, depending upon their masses, and then declining to cold and dark bodies, the whole series of changes requiring (at least until almost the present time) only a few tens of millions of years. Hale whole-heartedly adopts the same ideas. He says in 1922 (*op. cit.*, pp. 53–54):

> Stars in an early stage of their life history may be regarded as diffuse gaseous masses, enormously larger than our sun and at a much lower temperature. . . . Their density must be very low, and their state that of a perfect gas. . . . In the slow process of time they contract through constant loss of heat by radiation. . . . Finally comes extinction of light, as the star approaches its ultimate state of a cold and solid globe.

Creation at one end and stagnation and death at the other, unless a new creation rejuvenates the system! What simplicity!

In striking contrast with the foregoing, consider the planetesimal hypothesis. The fundamental point of view adopted in it is that the stars of our galaxy constitute a group of mutually related objects, the evolution of each depending in part upon its relationships to the others. They mix and mingle with one another, in the course of time, somewhat like molecules in a gas. At the time of the dynamic adventure of a suitable near approach of one star to another, planets are born from the parent suns. These planets grow up around nuclei by the accretion of countless little planets (planetesimals) born at the same time. Not only in the broad sweep of events leading to the birth of the planets as independent objects does this theory differ completely from the Laplacian, but also all the dynamical considerations involved in the growth and evolution of the planets are wholly different. More than one commentator on the planetesimal hypothesis has regarded with favor the origin of the planets by dynamic approach as being likely, and has then utterly failed to realize that the growth and evolution of the planets could not have been along the lines that are consonant with the Laplacian theory. The new hypothesis gives an entirely new earth and lays down a new basis for the development of dynamic geology.

A moment's consideration shows that the intervals of time required for the events contemplated by the planetesimal hypothesis are of an entirely different order from those that have been current in connection with the Laplacian theory. Instead of tens of millions of years, thousands of millions of years are necessary. Astronomers naturally could not feel at home in the order of ideas that underlie the planetesimal hypothesis until they grew out of the inherited conceptions of the restrictions of time imposed by the contraction theory of stellar energies. Some detailed quotations bearing upon this point will be given, both because of its fundamental relationship to the problem under consideration, and also because of some astonishing recent claims respecting priority in an order of ideas that is rapidly coming to be looked upon with favor by astronomers.

In 1899 Lord Kelvin gave an address, "On the Age of the Earth as an Abode Fitted for Life" (SCIENCE, May 12, 1899, pp. 665–674, and May 19, pp. 704–711). His basis for conclusions was the simple Helmholtzian contraction theory of the sun's heat, which provides a lifetime for the earth of the order of 25,000,000 years. With a dogmatism that the entire history of philosophy and of science and a realization of our own ignorance should warn us against, he marked

out definite time limits within which he asserted all terrestrial phenomena must be included. In SCIENCE for June 30, 1899, pp. 889–90, and July 7, pp. 11–18, Professor Chamberlin challenged both Kelvin's premises and his conclusions, and the events have shown that the challenge was fully justified. No one now would take seriously what Kelvin called "sure assumptions," "certain truth," and "no other possible alternative." After putting the date of the surface cooling of the earth between twenty and forty millions of years in the past, Kelvin speaks of "one year after freezing," "half an hour after solidification," and a "crust of primeval granite" having a depth of "several centimeters." I do not wish to emphasize so much the fact that Professor Chamberlin challenged the ideas on which the now passing generation of astronomers was reared, as to insist that he anticipated by nearly thirty years the basic ideas concerning the time-scale of the cosmic processes that are now rapidly winning favor. In 1899 (*op. cit.*, p. 889), he gave expression to the following startlingly prophetic words:

> Here [in the work of Kelvin] is an unqualified assumption of the completeness of the Helmholtzian theory of the sun's heat and of the correctness of deductions drawn from it in relation to the past life of the sun. There is the further assumption, by implication, that no other essential factors entered into the problem. Are these assumptions beyond legitimate question? In the first place, without questioning its *correctness*, is it safe to assume that the Helmholtzian hypothesis of the heat of the sun is a *complete* theory? Is present knowledge relative to the behavior of matter under such extraordinary conditions as obtain in the interior of the sun sufficiently exhaustive to warrant the assertion that no unrecognized sources of heat reside there? What the internal constitution of the atoms may be is yet an open question. It is not improbable that they are complex organizations and the seats of enormous energies. Certainly, no careful chemist would affirm either that the atoms are really elementary or that there may not be locked up in them energies of the first order of magnitude. No cautious chemist would probably venture to assert that the component atomecules, to use a convenient phrase, may not have energies of rotation, revolution, position, and be otherwise comparable in kind and proportion to those of a planetary system. Nor would he probably feel prepared to affirm or deny that the extraordinary conditions which reside in the center of the sun may not set free a portion of this energy. . . .

That Professor Chamberlin should have so exactly anticipated the discoveries of the last twenty-five years in subatomic structures and energies is extraordinary and to some extent accidental. But that he should have clearly perceived that there are vast sources of energy not theretofore taken into consideration was not accidental. It was an inescapable conclusion from several classes of facts that were then well established. The great age of the earth, as proved by geological evidence, made drafts on the bank of time far beyond the capacity of the contraction theory to meet; and the known scale of the stellar system made it as absurd to assume that stars are born and evolve and die in a few million years as it would be to assume that mountains are raised up to the clouds and are washed away in a summer's day.

For more than twenty years following 1899 nearly the whole scientific world continued serene in its inherited ideas respecting sources of energy and the time-scale of geologic and cosmic processes. Now, since 1920, the tide has set in toward the adoption of a time-scale measured by thousands—or even hundreds of thousands—of millions of years, and strangely enough the change is taking place without the discovery of any essentially new facts or the development of any new formulae bearing on the subject. The significance of the pertinent facts and of the formulae long available is just now being apprehended, and the conclusions from them are only now becoming mental property.

The main point that it is desired to make here, however, is that these ideas respecting energy and time, which are essential to the planetesimal hypothesis, have been held consistently by Professor Chamberlin since about 1900, or for more than twenty years before scientific men generally made them a part of their scientific thought.

Volume II of Chamberlin and Salisbury's "Geology" was published in 1905. Ideas respecting the duration of the sun that are essentially the same as those which astronomers are now adopting were set forth on pp. 51–2 in the following words:

> That self-compression is a potent source of heat is not questioned, but the Helmholtzian theory takes no account of sub-molecular and sub-atomic sources of energy. The transcendent potency of these sources of energy has been for some time suspected, and is now being revealed by refined physical research. The extraordinary energies displayed by radio-active substances are doubtless but an initial demonstration of immeasurable energies resident in other forms of matter and in the constitution of the sidereal system, and competent for its maintenance for unassignable periods. It does not appear, therefore, in the light of recent revelations in physics, or recent discoveries in the constitution of the stars and the stellar system, that there is any sufficient reason for setting narrow limits to the life of the sun. It seems more in accord with recent advances in knowledge to place the compressional theory of the sun's heat in the category of the earlier chemical and meteoritic theories, as true and contributory, but as only partial and inadequate. . . .

In the first edition (1906) of my "Introduction to Astronomy" (pp. 395–7 and 485–6), the same order of ideas was expressed in clearest terms. In the 1916 edition, pp. 360–4, 443–4, 495–8, they were repeated and amplified. Besides this, it was emphasized on pages 500–504 that the approximately steady state reached in the enormous globular clusters implies a dynamic evolution of these groups, and an existence of the stars of which they are composed, extending over thousands of millions of years. This argument is, in fact, the most conclusive we have.

The hypothesis that our planetary system may have had its origin in the dynamic effects of a passing star first began to be favorably considered by astronomers in about 1919. In that year Dr. J. H. Jeans published his "Problems of Cosmogony and Stellar Dynamics." His introductory chapter contains an excellent brief summary of theories of cosmogony, and on p. 16 he writes: "The most complete form of tidal-action theory is found in the 'Planetesimal Theory' of Chamberlin and Moulton." He had not, however, at that time changed his old ideas respecting the time-scale of cosmical processes. On p. 17 he states:

> Perhaps the most obvious criticism that can be brought against this and all other tidal theories is that they require the close approach of large astronomical bodies, and that such close approaches are very rare events. Calculations which will be given later seem to show that this consideration must lead to the abandonment of all tidal theories, including the planetesimal, as explanations of *normal* cosmogonic processes.

In order to avoid this difficulty respecting the time required and other embarrassments of the same sort, he made the astounding conjecture that our galaxy of stars has only recently expanded to its present dimensions—an assumption which Professor MacMillan showed in his review of Dr. Jeans's book[3] violates the dynamical assumptions upon which Dr. Jeans's reasoning was based. In discussing the time-scale, Dr. Jeans says (in 1919) on p. 286: "It hardly appears probable that the sun can have other sources of energy comparable with its gravitational energy." And on p. 289:

> We conjecture that something like 300 million years ago our sun experienced an encounter of this kind. . . . At this epoch the sun is supposed to have been dark and cold, its density being so low that its radius was perhaps comparable with the present radius of Neptune's orbit.

Dr. Jeans appears now (1928) completely to have reversed his ideas respecting the time-scale. In the *Annual Report* of the Smithsonian Institution for 1926, in an article entitled "The New Outlook on Cosmogony," he writes:

> The ages we must now attribute to our sun and the other stars are many hundreds of times longer than was, until quite recently, thought probable or even possible [by Jeans and his followers]. This extension of the time-scale will call for a rearrangement of ideas in many departments of cosmogony and astronomy.

There is another reason why the planetesimal hypothesis has not rapidly won favor. Although to discuss it is to digress somewhat, the point to which I refer is so important in the present connection and to the progress of science in general that I shall venture to make a few comments upon it.

It seems to be an almost universal human characteristic to demand a formula to explain things. In most matters the formula is a combination of words constituting some so-called principle or law; in physical science, it is generally a mathematical expression. The simpler the formula is, the more highly it is likely to be regarded. Presently it is not alone an epitome of past experience and of our knowledge; it becomes, as it were, something inspired, something that gives us complete truth and leaves us comfortably free from uncertainties. Our scientific literature is filled with statements that this law or that law "governs" a certain class of phenomena. Now, what is a law of nature? It is not something ultimate and of divine origin. On the contrary, it is only our formulation of the way we have perceived certain phenomena; it is subject to all the imperfections of our knowledge and to all our inherited and acquired prejudices. It seems clear from the nature of the case that no law of nature or formula that we shall ever construct will be universally applicable. Yet we all strive for the formula as though it were more than it is, and we distrust anything for which a formula has not been made. There are many instances in the work of Dr. Jeans where he has arrived at a formula, derived at great labor on the basis of a whole series of uncertain assumptions, where general commonsense reasoning would have been safer.

The foregoing general statements are excellently illustrated by theories of the source of the energy radiated by the sun. The formula of Helmholtz, which is simplicity itself, speedily found wide acceptance. It held such sway over the minds of scientists that it seemed completely to paralyze progress for twenty years after it should have been regarded as inadequate. Only when the Einstein formula for the relation between mass and energy gradually became known and could be made to take its place was the Helmholtzian formula abandoned. This appears to be the explanation of the complete change of position

[3] *Astrophysical Journal*, 51 (1920): 309–333.

of Dr. Jeans between 1919 and 1926. Certainly he considers no evidence in the latter year that was not fully available and that he did not discuss in the former.

We may now inquire on what authority we should accept unquestioningly Einstein's formula and apply it, with no misgivings, to the most far-reaching speculations. It was not given us by the Almighty on tablets of stone; it is not even an absolutely necessary consequence of Einstein's general theory. And Einstein's theory itself has been verified in only a few classes of phenomena, in all of which large percentages of uncertainty remain. The tests that have been made of the theory certainly are not of a nature to justify us in concluding that now, contrary to all past experience, we have arrived at ultimate and absolute truth, expressible in a few mathematical symbols. Even if the general theory of relativity is substantially correct, there are reasons to doubt the general validity of the formula expressing the relation between mass and energy. The energy it provides is substantially equal[4] to the electrostatic potential energies of the electrons of which atoms are composed, and, therefore, if it is correct there can be no important sources of energy still unknown. Suppose the electrons are composite and contain internal energies of an order as much higher than those now known as the energies of the electrons are higher than those of the atoms formerly known, and that they may be transformed into electronic energies as electronic energies are in some cases transformed into molecular energies. The theory does not provide for these possibilities, and even, I think, probabilities. The statement that positive and negative electrons combine and disappear accompanied by the appearance of something entirely different, called radiant energy, is merely a formula in words that cloaks our ignorance and that is likely to stifle our curiosity.

If the planetesimal theory had been expressed by a formula, it undoubtedly would have been accepted more readily by many minds, even though the formula added nothing to its content or probability. But the planetesimal hypothesis can not be expressed by a simple formula, such as that in Genesis or that which describes the Laplacian theory. It involves many complexities and a wide range of auxiliary theories, and instead of closing a chapter of ideas it opens up volumes of new ones. In spite of this, Dr. Jeans and Dr. Jeffreys have attempted to put the planetesimal hypothesis into a formula under the name of the "tidal theory"; but, as I shall later point out, their formula does not rest upon a substantial foundation.

[4] Wm. D. MacMillan, "Some Mathematical Aspects of Cosmology," SCIENCE, 62 (1925): 125.

In the second part of his book, Professor Chamberlin takes up the development of the planetesimal hypothesis, and an outline of the history of its development is important for later parts of this review. The first published approach to its underlying ideas was Professor Chamberlin's "The Possible Function of Disruptive Approach."[5] This paper was referred to with some degree of approval by Alfred Russell Wallace in his "Man's Place in the Universe" (1903; p. 185). In the Fairchild revision edition of LeConte's "Elements of Geology" (1903; pp. 293–4), it was also considered; and still again, in Miss Clerke's "Problems in Astrophysics" (1903; p. 445). In 1905 the second volume of Chamberlin and Salisbury's "Geology" (pp. 38–81), contained a full discussion of the planetesimal hypothesis. In the same year I described it in the *Astrophysical Journal* (22: 165–181). It was also treated in detail in the first edition of my "Introduction to Astronomy" (1906; pp. 463–487). From this time on, references to the planetesimal hypothesis appeared frequently in astronomical and geological literature. For example, Sir George Darwin, in his third (1911) edition of "The Tides" (pp. 412–426), described the planetesimal theory with keen penetration respecting its bearing upon scientific thought and with gratifying cordiality. In an earlier part of the final chapter, he went far toward abandoning the Laplacian theory, which he had always theretofore accepted, and he closed his book with the following paragraph:

> The authors [Chamberlin and Moulton] frankly admit that their hypothesis [the planetesimal] may need revision in many respects, and this is no doubt inevitable in so ambitious an attempt. Whatever be its fate they are to be congratulated on having advanced views of extraordinary interest; and whether the theory be sound or not in all its parts they have made a contribution to cosmogony of great importance.

By this time the planetesimal hypothesis was generally recognized by geologists as being a hypothesis worthy of respect and consideration. For example, Pirsson and Schuchert expounded it on pages 530–7 of their "Geology" (1915).

Dr. Jeans appears to have referred to the planetesimal hypothesis first in 1919 in his "Problems of Cosmogony and Stellar Dynamics," fourteen years after it had appeared in leading scientific magazines and in Chamberlin and Salisbury's "Geology" and in my "Introduction to Astronomy." In an introductory chapter, under the sub-heading "The Tidal-Action Theory," he states: "The most complete form of tidal action theory is found in the 'Planetesimal

[5] The *Astrophysical Journal*, 14 (1901): 17–40, and the *Journal of Geology*, 9 (1901): 369–392.

Theory' of Chamberlin and Moulton." In mentioning it, however, he gives no reference to its early publication or dates, but contents himself with citing generally Chamberlin's "Origin of the Earth," which he points out was published in 1916. A reader unacquainted with the facts from independent sources would be likely to infer that the work of Professor Chamberlin did not antedate much, if any, that of Dr. Jeans. In this section he explains that the theory involves the close approach of one star to another with resulting tidal forces and the generation of a spiral formation. In his concluding (XII) chapter, entitled "The Origin and Evolution of the Solar System," the Laplacian theory is examined and disposed of as Professor Chamberlin and I disposed of it in 1900 and for substantially the same reasons. The "tidal theory" is developed at some length with no reference whatever to the planetesimal hypothesis. In three later publications Jeans has explained the so-called tidal theory, and in none of them has he made any reference, direct or indirect, to the planetesimal theory or to Chamberlin. One of these papers is published in the *Annual Report* of the Smithsonian Institution for 1924; another is in the same publication for 1926; and the third is in a collective work to which further reference will be made.

Dr. Harold Jeffreys entered the field of the planetesimal hypothesis in several papers which appeared in the *Monthly Notices of the Royal Astronomical Society* in 1916–1918. These discussions, revised somewhat as a consequence of the work of Dr. Jeans, are included in his book, "The Earth," which was published in 1924.

It follows from the foregoing historical sketch that it is an inexcusable violation of the facts to state or imply that Dr. Jeans was the author of the theory that the planets had their birth at the time some star passed near our sun. The theory had appeared in many publications and had been presented to thousands of students of geology and astronomy for more than ten years before Dr. Jeans published a word upon the subject. The position taken by Drs. Jeans and Jeffreys, which has spread to others, as is illustrated by the sentences quoted from Dr. Eddington at the beginning of this review, is an astounding phenomenon. The way in which the planetesimal hypothesis faded from view between the first and last chapters of Dr. Jeans's book has been noted. But in the book of Dr. Jeffreys, it is the tidal theory throughout the text, with the planetesimal hypothesis relegated to an appendix, except for a brief reference in Chapter II. In sharp contrast with his treatment of the planetesimal hypothesis, Dr. Jeffreys devotes the entire first chapter to the Laplacian theory, which he rejects on essentially the grounds developed by Chamberlin and me in 1900, but with no reference to our work. In the matter of credit for essential ideas, the book of Dr. Jeffreys appears to be unique. In scores of references to his own work and to that of Dr. Jeans and other British writers, he in all cases follows the usual custom of giving the name of the publication, the volume, the year of publication and the pages in the volume. Such information enables a reader to fix clearly in his mind the historical sequence of things to which references are made, and it enables him easily to examine any original sources he may wish to consult for details. But nowhere in the book of Dr. Jeffreys is there a single corresponding explicit reference to the work of Professor Chamberlin or to my own writings. In not a single instance is a specific date given and in only three instances are the correct titles of the publications given, and they are titles of text-books without the dates of publication. There is no reference whatever to original publications in scientific journals. To such an extent has the "made in England" been pushed that Dr. Jeffreys, like Dr. Jeans, ascribes Helmholtz's contraction theory of the sun's heat to Lord Kelvin, and he makes no reference whatever to Helmholtz in his book. He has several chapters on tidal and other deformations of the earth, but he remains completely silent on the incomparable earth-tide experiments of Professors Michelson and Gale.

Thirteen distinguished British scientists and philosophers wrote "Evolution in the Light of Modern Knowledge," which was published in 1925. Dr. Jeans is the author of Chapter I, "Cosmogony," and Dr. Jeffreys wrote Chapter II, "The Evolution of the Earth as a Planet." Since the book is a non-technical discussion of a broad field, suitable for the intelligent general reader, it can not justly be expected to contain exhaustive references to original sources. Yet there are references in the text, and in bibliographies at the ends of the first two chapters, to Lucretius, Descartes, Swedenborg, Kant, Thomas Wright (of Durham, England), Laplace, Babinet, Dr. Eddington, Darwin (Sir George), and especially to Drs. Jeans and Jeffreys; there is a two-page discussion of the theory of Kant, an equally long discussion of the Laplacian theory and a four-page discussion of the tidal theory; but the planetesimal hypothesis and Chamberlin and Michelson and Einstein do not exist.

In view of these astounding tactics, the very least that could be expected is that Drs. Jeans and Jeffreys should have developed a tidal theory that in no way had its origin in the planetesimal hypothesis and that in its essentials was entirely distinct from the plane-

tesimal hypothesis, or that they should have established conclusions that were of the very first order of importance. Let us examine the facts, first as to whether the so-called tidal theory descended from the planetesimal hypothesis. Dr. Jeans says, in his "Theories of Cosmogony" (p. 17): "The most complete form of tidal-action theory is found in the 'Planetesimal Theory' of Chamberlin and Moulton." In Appendix A, page 251, Dr. Jeffreys says: "The Planetesimal Hypothesis was historically the parent of the Tidal Theory of the origin of the Solar System, elaborated in Chapter II." That is, originally both Dr. Jeans and Dr. Jeffreys seem to have acknowledged that the tidal theory in some real sense was a direct descendant of the planetesimal hypothesis.

Let us compare the theories themselves. The planetesimal hypothesis ascribes (1903 and later) the birth of the planets to the dynamic effects of a passing star; the tidal theory (1919 and later) does exactly the same thing. The planetesimal hypothesis ascribes the separation from the sun of the materials of which the planets are composed to the combined effects of the tides generated in the sun by the passing star and to eruptive activities such as now are exhibited by the sun; the tidal theory ascribes the separation to the effects of the tides alone, at a time when the sun was much larger than it is at present. (This point will be discussed further.) The planetesimal hypothesis ascribes to this origin of the planets the relatively small amount of momentum of the solar system and its significant distribution between the planets and the sun; the tidal theory draws exactly the same conclusion in exactly the same way. The planetesimal hypothesis explains the fact that the planetary orbits are nearly coplanar, and the fact that the planets all revolve about the sun in the same direction, to the cross-component attractions of the passing star in the plane of its orbit; the tidal theory draws exactly the same conclusions in exactly the same way. The planetesimal hypothesis assumes that the planets have grown up about nuclei in the matter that left the sun, the exact characteristics of the nuclei being at present undeterminable; the tidal theory assumes that the planets grew up around nuclei and maintains that it has been proved that the nuclei were liquid, not solid, almost immediately after they left the sun. The planetesimal hypothesis explains the small eccentricities of the orbits of the planets as the effects of their growth by collisions with planetesimals; the tidal theory does the same except that Dr. Jeffreys claims to have shown that if the planetesimals were not originally molecular they would be reduced to that state and would have the properties of a gaseous resisting medium. The planetesimal hypothesis ascribes the present rotation of the sun to its original rotation and to the effects of the planetesimals that fell back upon the parent body; the tidal theory of Dr. Jeffreys makes exactly the same explanation. The planetesimal hypothesis explains the satellites as bodies that have grown up about secondary nuclei accompanying the original planetary nuclei or later becoming entangled with them; the tidal theory ascribes the satellites to matter tidally removed from the planetary nuclei by Jupiter and by the sun. Professor Chamberlin concludes that the earth has grown almost entirely from solid bodies and that it has been solid through and through, as it is now, during nearly all its growth; Dr. Jeffreys believes the earth-nucleus was liquid almost from the time it left the sun and that it grew quickly to its full size in the liquid state. Thus, in every essential concept the two theories are identical; yet Dr. Jeffreys sets them forth, point by point, in his Chapter II (*op. cit.*, 1924), "The Tidal Theory of the Origin of the Solar System," with not a word to suggest that they all had been published in detail nearly twenty years earlier. On the other hand, he is meticulous in acknowledging credit for things of much less importance to his discussion.

Now let us return to the tidal theory in which Drs. Jeans and Jeffreys claim that the planetary materials were separated from our sun by the tidal effects alone of a passing star. Let it first be noted that both Dr. Jeans and Dr. Jeffreys ignore the fact that comets, having very small masses and large dimensions, have been observed to pass very close to the surface of the present condensed (according to them) sun without undergoing the tidal disruption that they imply they have proved would necessarily follow, even in the case of much less tenuous stars approaching less closely a comparable tide-raising body. Since the tide-raising forces vary inversely as the cube of the distance from the tide-raising body and directly as its mass and the radius of the disturbed body, it follows that Drs. Jeans and Jeffreys would have us believe that tidal forces ten thousand times less than those to which comets have been subject without serious damage have quickly torn great Jupiter and Saturn from the side of our sun.

Drs. Jeans and Jeffreys have put a considerable part of their discussion in mathematical terms and have wholly neglected the naturalistic method of approach. As a consequence, they have considered certain questions for which, on the basis of sufficient assumptions, formulae could be developed, and they have ignored the remainder. Among other things, they have given formulae and a diagram in explanation of the separation of the planetary masses from

the sun under the influence of the tidal forces of a passing star. Dr. Jeans set up the equipotential surfaces for the force system of the sun and the visiting star substantially as I set up the same conditions in 1900[6] in examining the stability of the Laplacian ring. My conclusion was that when the surfaces were open the ring would not contract into a dense mass. Drs. Jeans and Jeffreys assert that when the equipotential surfaces were dumb-bell shaped, all the material of which the planets are now constituted "are shot away from the primary star" (Jeans) through the opening (the dumb-bell handle) from our sun toward the visiting star. They assign no reason for assuming that the motion would be from the sun toward the star rather than from the star toward the sun. They do not even attempt a determination of the rate that material would be "shot" in this way under any conditions, actual or hypothetical. They ignore the fact that the resultant gravitational accelerations are normal to the equipotential surfaces and not parallel to them, and that, consequently, the tendency to flow through the spout is not gravitational. They assume that matter, to escape from one star, must flow out from it through the spout, and then neglect to explain why it does not belong to the other star. They make no reference to the fact that as the visiting star recedes the dumb-bell breaks and leaves the "tidal filaments" interior to one of the ovals. They speak as though the cross-components of motion of the filaments were tidal effects, though in tides the wave-form and not the matter moves. Hence, even if they were wholly correct, it would be difficult to justify their title for the theory. They make no quantitative discussion of the short time the visiting star must have been near the sun. They speak of "slow encounters" and "transitory encounters," and Dr. Eddington in his recent paper (*loc. cit.*) speaks of the star having slowly overtaken or been overtaken by our sun; all in spite of the fact that the star came from stellar distances and has receded again to stellar distances, from which it follows that the relative velocity of the sun and the star must necessarily have at least equaled the parabolic limit. In short, they have not even remotely approached a mathematical demonstration of the validity of their assumption that tidal forces alone will account for the separation of the planetary material from an ancestral sun. And corresponding statements apply to their suggestions respecting the origin of the satellites and the early states of the planets.

I do not make the foregoing remarks in a harshly critical spirit, for the field of their discussions is beyond the range of sound mathematical treatment. The fact that their formulae and diagrams do not approach logical conclusiveness is of no consequence except in so far as the formidable appearance of these mathematical tools misleads the unsophisticated. Even if their conclusions were unimpeachable, I should not regard them as being of great significance, for they rest on assumptions that may not correspond with the facts. Let me make a much more general statement respecting theories in the domain of physical science. When a theory has been definitely formulated, it may be examined mathematically in order to determine, so far as may be possible, whether its various parts are consistent with one another and with other accepted facts and theories. If inconsistencies are brought to light, the theory must be modified or abandoned. If no inconsistencies are revealed, the theory may be somewhat more probable, but it is still uncertain. That is, mathematical processes may disprove a physical theory, but they can never completely establish one. For example, in 1900 Professor Chamberlin and I brought out fundamental inconsistencies in the Laplacian theory and we abandoned it. But we could not logically prove the correctness of the planetesimal hypothesis, nor have Drs. Jeans and Jeffreys succeeded in proving its correctness, or even approached such a proof.

As has been pointed out, a mathematical formula is not sacred and has no greater validity than the assumptions upon which it is based. Common sense supports this position and the whole history of science warns us against placing great faith in a formula, whether it be in words or in mathematical symbols. The Helmholtzian contraction theory of the heat of the sun excellently illustrates the point. The assumptions on which a formula is based are not only those that are explicitly expressed, but they are also those which are subconsciously held by its author and which make up his general point of view. It has already been remarked that Dr. Jeans in the *Annual Report* of the Smithsonian Institution for 1926 reversed his earlier views on the duration of the stars without using any new data. Dr. Eddington did the same thing in an article, "The Borderland of Astronomy and Geology," published in *Nature*, January 6, 1923, and reprinted in the *Annual Report* of the Smithsonian Institution for 1923. In the text, Dr. Eddington says: "There must have been a time when the sun's heat was from twenty to fifty times more intense than it is now." But in the Smithsonian reprint there is added the footnote: "New facts have emerged since this was written. I think we can now say fairly definitely that the sun's heat has not altered appreciably during the last ten thousand million years." One familiar with this field wonders what

[6] *Astrophysical Journal*, 11: 122–126.

"new facts" emerged in the interval of a few months to justify this astounding reversal of opinion, except that Dr. Eddington had changed his point of view.

The assumptions that underlie one's point of view are not easily recognized by the worker himself. If he overlooks their importance, the fact may be regarded with charity, for the fault is common. But it is not too much to expect that he will keep steadily in his own mind and place fairly before his readers the fact that his conclusions are as uncertain as his definitely expressed assumptions. He should not proceed as though the probabilities of uncertain hypotheses, sequentially introduced, compound by addition rather than by multiplication. The conclusions of Drs. Jeans and Jeffreys and Eddington are not usually characterized by conservative formulation. For example, in his "Problems of Cosmogony" (1919), under the subheading "The Time-Scale," Dr. Jeans writes on page 287:

> Taking the luminosity of the average star to be 1/10, we find that the contraction provides for radiation at this rate for 530 million years, a period which agrees well enough with our other estimates of the age of the universe.
>
> Thus as regards the universe as a whole, there is no difficult problem associated with the time-scale: the problem only arises in connection with special stars, and our sun happens to be one of these.

Would he now assert there is no difficulty in the time-scale he then held?

Then in 1925,[7] in discussing the same question and the same data, he says:

> The length of time to bring about an imperfect approximation such as is observed is found to be of the order of millions of millions of years. Although we can not say that any individual star has lived for this length of time, we can be fairly confident that the great majority of stars have done so.

Millions of millions of years! Fairly confident! One may inquire what in all the realm of science should be regarded as merely a hypothesis and held cautiously.

For about fifteen years Drs. Jeans and Eddington have been making persistent, skilful and praiseworthy efforts to penetrate the difficult field of the internal constitution of the stars. Although it has been necessary for them to pile one assumption on another, at every stage of their work they have appeared to be confident of the substantial correctness of their results. Now, in *Monthly Notices of the Royal Astronomical Society,* for October, 1927, on pages 724–5, Dr. Jeans states that all their work had been based on an erroneous assumption respecting radiation pressure, and he concludes:

> This invalidates the whole of the discussions, and any apparent success they [the discussions] may have achieved must have been purely fortuitous. In liquid stars radiation pressures may in general be disregarded and the luminosity mass-temperature relation must be examined *de novo.*

So far as I know, for the time being the matter rests there.

The foregoing remarks have been made partly for the purpose of illustrating the danger of accepting results simply because they are clothed in more or less of mathematical garb, and partly for the purpose of throwing in sharp relief the naturalistic methods employed by Professor Chamberlin. The third major division of his book is devoted to the nature, the origin and the evolution of meteors, meteorites and comets. In a field beset with enormous difficulties, Professor Chamberlin exhibits skill of the highest order in seizing on significant facts and analogies; he pursues every hopeful clue; he brings into view and examines all promising hypotheses; he is bold in inventing new hypotheses, and he presents his conclusions as results to be tested by time and new discoveries. Even though one should disagree with some or many of the conclusions Professor Chamberlin regards as possible, yet one can not but learn much from his discussions and feel the stimulating effects of following a daring mind in its wide excursions in unfamiliar domains. However strange and strained some of his views may seem now, it may very well be that in thirty years he will be found to be now as much in advance of his times as it is now known that thirty years ago he was in advance of his times in respect to the heat of the sun. The differences in point of view are in this case no less radical and startling than they were in the former. What I wish to insist on is only that the fact that they depart from current ideas is not real evidence against their correctness, and the daring excursion will certainly benefit science. When considered broadly, Professor Chamberlin's contribution to the methods of thought in the field of cosmogony will be regarded as highly as the new ideas he has advanced.

In a time when many have talked of the creation and the final death of the physical universe and have made such concepts basic in their thought, he has entertained no such philosophic juvenilities, nor has he insisted on theories expressed in closed formulae, nor has he been depressed by the thought that much is unknown. It seems that many minds have a sort of horror of an unending past or future, or infinite

[7] "Evolution in the Light of Modern Knowledge," p. 19.

space, and that to avoid the terrors of open time they assume a creation, wholly unconscious of the profound and wide-reaching implications of the assumption.

For example, Dr. Jeans closes his paper in the October, 1927, number of the *Monthly Notices of the Royal Astronomical Society* with the astounding sentence: "As a corollary, it would be difficult to deny that all the matter of the universe may have been created at the same instant." To ascribe to such a conclusion the logical relationship of a "corollary," and especially to entertain it on any grounds whatever, means that in spite of the fact that Dr. Jeans appears to adopt the planetesimal hypothesis under the name of the tidal theory, and in spite of the fact that he has recently changed to the general order of ideas respecting the time-scale of the cosmic processes held by Professor Chamberlin since 1899, the fundamental philosophic point of view occupied by him and his followers is separated from that of Professor Chamberlin and his associates by an immeasurable gulf.

F. R. MOULTON

CHICAGO, ILLINOIS

OF HUMAN INTEREST

To
JOHN HENRY COMSTOCK
and to
ANNA BOTSFORD COMSTOCK
On this their Golden Wedding Day
Sunday, October seventh, 1928:
Be
Peace and joy and every happy memory
Though but a few of all the many who in these fifty years have felt the benediction of their home, we cannot let this anniversary day go by without our word of love and gratitude.

SUCH was the wording of a beautiful illuminated testimonial scroll signed by a group of immediate personal friends of the Comstocks and presented to them on Sunday morning, October 7.

On the preceding day the Ithaca *Journal-News* had given editorial expression to the esteem of the community, as follows:

> Cornell and Ithaca need no special occasion to remind them of Professor John Henry Comstock and Mrs. Comstock. The lives of both are too closely and too happily interwoven with that of the University and city communities to need such a reminder; but all will rejoice with them to-morrow on the occasion of their golden wedding anniversary, and many will take pleasure in recalling kindnesses received at the hands of these distinguished citizens.

Then after an enumeration of the books that they both have written and of the high honors that have come to both of them, the editorial continues:

> Their home has been a center of hospitality for a generation, and many boys and girls from the country have found comfort and inspiration in its atmosphere. No one will ever know how many careers have been formulated, how many lives have been given an upward bent through association with these two.

The editor further went on to express the

> Sense of gratitude felt in their home city for the half century in which they have lived together to the great enlightenment of their chosen scientific field, for the benefit of the community and for the enrichment of the lives of their many friends.

Similar tributes of esteem were editorially expressed in the *Cornell Daily Sun* and in the *Alumni News*. The latter said:

> They are a couple unique in university and scientific circles. Both are internationally known in their respective fields of science. With the exception of a few years devoted to study and to teaching in other institutions, they have been connected with the University since 1869.

Thus, during nearly the entire history of Cornell University they have lived and worked together. In the fullest sense of the word they have been colaborers. Their respective fields of labor, entomology and nature study, broadly overlap, and nothing in the province of the one has ever been too sacred for the use of the other, if needed. And back of the work and sustaining the work, there has always been a home of good cheer and genuine friendliness.

Professor and Mrs. Comstock, though stricken in health with the burden of the years, still live in their beautiful home among the trees on the brink of the gorge above the Primrose Waterfall. Here they are surrounded by personal treasures that have come to them from all over the earth. Here was the Mecca of many distinguished entomologists who came to the recent International Congress, bringing new tributes of respect. Here, also, their hospitable door is still open to the humblest student, and for him there is unending friendliness and encouragement within.

Their lives are still devoted to sound learning and sane living.

JAMES G. NEEDHAM

CORNELL UNIVERSITY

SCIENTIFIC EVENTS

EXHIBIT OF MINERALS IN ULTRA-VIOLET LIGHT

THE London *Times* reports that by means of a mercury vapor lamp, which has been installed in the Mineral Gallery of the British Museum (Natural History), Cromwell Road, members of the public, by pressing an electric button, can flood a case containing fluorescent minerals with invisible ultra-violet light and see the remarkable phenomenon which results.

Most of these minerals are practically colorless, but as soon as they are exposed to ultra-violet light they absorb this invisible light and emit in their stead visible rays of longer wave-length. Fluorspar, a brown mineral, changes color, and becomes a bright violet. Colorless quinine shows up bright sky-blue, zinc-silicate becomes green, and in some cases yellow; zinc-blende is changed to a shining mass, old-gold in color, and in some cases a brilliant fiery yellow, like a live coal. A pale-green slab of willemite glows with remarkable brilliance, the color being comparable with uranium-green. Another specimen, with grains of pale-green willemite embedded in a cleavage-mass of white calcite, is changed to dark flesh-red color, in which are spots of brilliant green, due to the willemite.

The installation of this ultra-violet light in the museum has been carried out more with the idea of providing a novelty show-case than for scientific purposes. The principle of the use of ultra-violet rays in the detecting of certain minerals, however, is being explored by Dr. L. J. Spencer, keeper of minerals in the museum.

It has already been discovered that the rays show similar effects on a variety of oils and certain papers, and there is a possibility that practical use may be made of these phenomena. In a statement yesterday, Dr. Spencer said that during a visit to the mineral locality of Franklin Furnace, New Jersey, he saw a simple arrangement used by the New Jersey Zinc Company for the quick detection of willemite. This was done by making use of the ultra-violet rays emitted from a high-tension spark. It occurred to him that this might prove a simple means of detecting willemite in Rhodesia, where there were large quantities, but he found, on experiment, that willemite did not always glow when placed under ultra-violet light. It became evident from the tests he carried out that fluorescence in ultra-violet rays was not a constant and essential character of any mineral species.

In spite of this difficulty, Dr. Spencer was of the opinion that ultra-violet rays might prove of practical use in a variety of respects in the detection of minerals. He thought that it might be used underground for the rapid detection of certain ores. It could also be used to detect forged bank notes or postage stamps, as different qualities of paper glowed different colors when exposed to the rays. It could be used in the cement industry for the detecting of the valuable "slag" from the waste heaps of iron furnaces. The slag which was suitable for the manufacture of cement or for road-making glowed a purple tint, while the valueless slag became green.

MUSEUMS IN THE NATIONAL PARKS

OPPORTUNITIES to combine educational advantages with a pleasurable vacation are being forwarded by the National Park Service of the Department of the Interior in cooperation with leading educational organizations of the country.

During the past summer the Secretary of the Interior requested a number of prominent educators to make a thorough study of and report on the educational possibilities of the national parks. Funds to cover the expenses of the investigators were provided by interested organizations which have taken a keen interest in the development of the educational and museum work in the parks. Drs. John C. Merriam, Hermon C. Bumpus, Frank R. Oastler, Vernon Kellogg and Harold C. Bryant accepted the secretary's invitation to make this educational survey, and during the summer field inspections were made of the major national parks. Definite recommendations based upon their field studies will be made by the investigators later in the year.

Study of museum conditions in the national parks by a committee of the American Association of Museums led to that organization securing a grant of $118,000 for the construction of museums in Yellowstone National Park, the funds to be expended under a committee composed of Dr. Hermon C. Bumpus, of the American Association of Museums; Dr. Frank R. Oastler and Superintendent Horace M. Albright, of the park. Plans include the construction of a new museum in the headquarters group at Mammoth Hot Springs, a branch museum and auditorium at Old Faithful, several smaller buildings, some exhibits in place, and other features of permanent educational value. Construction has already been started on the Old Faithful Museum, which is expected to be ready for service during the season of 1929.

Of importance in the educational world was the completion of the trailside museum and observation station at Yavapai Point in the Grand Canyon National Park. This was formally opened to the public

on July 17 by Dr. Merriam. The structure was built and equipped through the cooperation of a number of educational organizations, among which were the Carnegie Institution of Washington, the Laura Spelman Rockefeller Memorial, the American Association of Museums and the National Academy of Sciences. Constructed of native weathered stone and logs, the station harmonizes with the landscape features and seems almost to be an integral part of the canyon walls.

The importance of establishing libraries in the national parks, both in connection with the educational work and for the use of the public generally, was established by the use of the library maintained in connection with the Yosemite Museum. Based on this, the American Association of Museums interested the American Library Association to such an extent that a committee was appointed to establish libraries in the national parks.

SERVICE OF THE WEATHER BUREAU TO AVIATION

METEOROLOGY in aid of aviation and marine meteorology—two of the many activities of the Weather Bureau of the U. S. Department of Agriculture—are discussed by Dr. C. F. Marvin, chief of the bureau, in his annual report. He says:

Since 1926 funds have been included in the regular appropriations of the department for assigning and maintaining on duty at every important airport one or more skilled meteorologists, whose duties require them to receive from the central organization of the Weather Bureau the fullest possible advices, reports, observations, etc., including forecasts and warnings, and to pass these on to pilots of airplanes at the time of and in accordance with the flight immediately in contemplation.

The basis of advices and warnings to pilots is necessarily derived from the great network of meteorological stations that has been built up by the Weather Bureau throughout past years. In some cases these stations are rather widely distributed. Moreover, observations are made regularly only at 8 A. M. and 8 P. M. For the needs of aeronautics, more intensive and special stations are required, especially in certain regions. To make provisions for this 137 ground stations have been established at frequent intervals all along the airways set up by the Department of Commerce, and the whole machinery of operation is gradually being improved to make the service more and more effective.

There is a growing demand for four daily observations from meteorological stations over the entire globe, instead of two. The hours for these observations are quite likely to be advanced in the near future in the United States so as to occur at 1 and 7 A. M. and at 1 and 7 P. M.

A few years ago the entire scheme for collecting by telegraph the meteorological reports from field stations was reorganized. A new and more flexible system is now in operation.

In no other field has the demand for meteorological help for aviation been more pressing than in connection with transoceanic air navigation. Agreements are under way with other great maritime nations for better organizing ocean meteorological observations by the selection of a certain number of ships of each nationality which shall uniformly make and distribute radio observations twice, or perhaps four times a day while on the high seas. . . . The highest efficiency and accuracy in formulating weather forecasts and warnings is only attainable when the meteorologist has before him a complete picture of the weather conditions over the whole surface of the globe, or at least over the whole surface of the Northern or Southern Hemisphere. The development of an international meteorological oceanic service along these lines is perhaps the most urgent technical problem concerning meteorology at the present time.

EPILEPSY COMMISSION OF THE HARVARD MEDICAL SCHOOL

A CITY-WIDE campaign against epilepsy has just been launched by the Harvard Medical School with the appointment of an Epilepsy Commission. Funds are now being collected by the commission, and research and experiment will be begun shortly at the Medical School and at various Boston hospitals in an investigation of a disease which now has 390,000 victims in the United States.

The commission, as appointed by the Corporation of Harvard University, contains the following members: Dr. Walter B. Cannon, Dr. Fritz B. Talbot, Dr. Bronson Crothers, Mr. Robert Amory, Dr. Stanley Cobb and Mr. Ralph Lowell. Mr. Charles Francis Adams is acting as treasurer for the commission in its drive for funds.

In a statement describing some of the "baffling problems" facing the commission in its campaign against epilepsy, Dr. Stanley Cobb, Bullard professor of neuropathology, writes as follows:

This commission has been appointed to promote a continuous study of the convulsive disorders over a period of years. The term epilepsy is used for brevity, but it has been demonstrated in recent years that epilepsy is not a disease—it is a type of reaction of the human body to different abnormal stimulations; it has various causes. Thus the field of study must be broadened to include the convulsions of childhood, the eclampsia of pregnancy, uremia, asphyxia and other allied conditions. When these are all better understood there will be more chance of helping the chronic sufferer—the epileptic.

It is estimated that there are 390,000 epileptics in the United States. This represents an enormous amount of suffering, especially when one realizes that not only the patient suffers but the whole family, for the fear of a catastrophe is ever hanging over the household, coloring the lives of all. The common convulsions of childhood are less distressing but more common; one survey showed that 7 per cent. of children had had convulsions before

the age of four. At a large epileptic hospital it was found that about half of the patients began their convulsions before the age of four. Just what the relationship may be between this acute condition and the chronic epilepsy of adults is apt to be elicited. Such are a few of the problems presented; their importance is obvious; they are the more challenging to medical science because they are so baffling.

During the last few years advances in treatment have been made. Chief among them is the discovery that acidosis tends to stop convulsions. Many children have been completely relieved by the practical application through diet of this chemical knowledge; in adults the diet is seldom of avail. It is obvious that the processes underlying these phenomena are not completely understood, and it is hoped that if a more complete understanding of them is obtained dietary treatment may be more universally successful. Another important problem is the relationship of the oxygen supply of the nerve cell to convulsive seizures. Recent work indicates that this may be the crux of the question. Studies concerning toxins absorbed from bacteria in the bowels are being carried out; such auto-intoxications may be the exciting cause of convulsions. Psychological factors are also important, and have been studied, but not extensively enough.

For more than five years work along these lines has been carried on at the Harvard Medical School, at the Massachusetts General Hospital, the Children's Hospital and at the Boston City Hospital. The appointment of the Harvard Epilepsy Commission makes possible a coordination and continuity of the work. No valuable results can be expected from research of this kind unless it be carried on for years, so the promotion of a permanent commission is a most important advance. Funds must be raised to carry on the investigations. At present about $10,000 a year is needed, but if generous support is given the scope of the work can be enlarged greatly.

FOREIGN SCIENTIFIC MEN AT THE U. S. FOREST PRODUCTS LABORATORY

The ranks of the foreign scientists working on American wood-utilization problems in the Forest Products Laboratory of the United States Department of Agriculture at Madison, Wisconsin, were augmented recently by the arrival of five men sent by government and private agencies in Australia, Finland, Poland and Sweden.

H. B. Somerset, Melbourne, Australia, will work as a member of the pulp and paper staff of the Forest Products Laboratory for a period of one year before returning to Australia to take a position in a paper mill operating on eucalyptus.

C. Ellis, forest economist to the Queensland Forest Service, Brisbane, Australia, will make his headquarters at the laboratory for the next twelve to eighteen months, studying its organization and methods, and using it as a point of departure for trips to various wood-using industries of the United States and Canada.

K. Kuoppamaki, mechanical engineer from Finland, has spent some time at the laboratory studying the manufacture of plywood.

Dr. J. Wiertelak, assistant in the institute of chemistry in the University of Poznan, Poland, is beginning a year of study at the Forest Products Laboratory on a scholarship of the Polish Ministry of Education. Dr. Wiertelak's studies will be principally on the chemistry of wood.

Carl Gustaf Strokirk, Harnosand, Sweden, is at the laboratory on a grant from the University of Commerce, Stockholm. Mr. Strokirk will remain at the Madison Laboratory until May studying the manufacture of plywood and other wood-utilization problems. During the summer of 1929 he will obtain employment in American woodworking plants to observe American methods. He will return to the laboratory next fall.

J. E. Cummins and H. E. Dadswell, Commonwealth (Australian) Council for Scientific and Industrial Research, are nearing the end of a two-years' study at the Forest Products Laboratory.

SCIENTIFIC NOTES AND NEWS

Officers of the Royal Society elected at the anniversary meeting on November 30 are: *President,* Sir Ernest Rutherford; *Treasurer,* Sir David Prain; *Secretaries,* Sir James Jeans and Dr. H. H. Dale; *Foreign Secretary,* Sir Henry Lyons; *Other Members of Council,* Dr. F. A. Bather, Dr. C. Bolton, Dr. C. G. Douglas, Mr. R. H. Fowler, Professor E. W. Hobson, Sir Frederick Hopkins, Professor A. Lapworth, Professor J. C. G. Ledingham, Professor F. A. Lindemann, Dr. P. C. Mitchell, Professor J. C. Philip, Professor A. C. Seward, Professor G. Elliot Smith, Sir Thomas Stanton, Mr. A. A. C. Swinton and Professor C. T. R. Wilson.

British scientific societies have elected presidents as follows: The London Mathematical Society, Dr. Edmund T. Whittaker, F.R.S., professor of mathematics in the University of Edinburgh; the Mineralogical Society, Dr. G. T. Prior, F.R.S., keeper of the department of minerals of the British Museum; the Cambridge Philosophical Society, Mr. G. Udny Yule, F.R.S., lecturer in statistics in the University of Cambridge, and the Philosophical Society of the University of Durham, Sir Charles A. Parsons, F.R.S., chairman of the engineering works of C. A. Parsons and Company.

Dr. Edward Francis, surgeon of the U. S. Public Health Service, who isolated the tularemia germ and

has been devoting intensive study to malta or undulant fever, has been striken with the latter disease, contracted in his studies at the Hygienic Laboratory.

A TESTIMONIAL dinner was given on November 20, at the University Club, Baltimore, in honor of Dr. William H. Welch, professor of the history of medicine and director emeritus of the Johns Hopkins University School of Hygiene and Public Health.

DR. WALTER BRADFORD CANNON, George Higginson professor of physiology in the Harvard Medical School, has been appointed exchange professor to France for 1929–30. His term will fall in the second half-year.

DR. JOHN STEWART, dean of the faculty of medicine, Dalhousie University, Nova Scotia, has celebrated his eightieth birthday.

THE George Robert White gold medal was awarded at the meeting of the Massachusetts Horticultural Society on November 8 to Colonel William Boyce Thompson, of Yonkers, founder of the Boyce Thompson Institute for Plant Research.

THE Paris Academy of Sciences has awarded the Albert de Monaco prize of 100,000 francs to Professor Cotton for the electro-magnet that he has devised and installed in his laboratory.

PROFESSOR G. VITALI, of the University of Padua, has been awarded the prize for mathematics for the year 1927 by the Italian Society of Sciences.

THE Gedge prize for original observations in physiology has been awarded to Dr. W. A. H. Rushton, of Pembroke College, Cambridge, where he holds a Stokes studentship for research in physics and cognate subjects.

THE officers of the division of biological chemistry of the American Chemical Society for the year 1929 are: *Chairman,* M. X. Sullivan; *Secretary,* D. B. Jones; *Executive Committee,* Paul E. Howe, G. H. A. Clowes, H. B. Lewis, L. S. Palmer and L. K. Riggs. The secretary of the division may be reached at the Bureau of Chemistry and Soils, U. S. Department of Agriculture, Washington, D. C.

THE Washington Section of the American Chemical Society has elected the following officers for the coming year: *Chairman,* R. Gilchrist; *Secretary,* Ruth O'Brien; *Treasurer,* E. G. Zies; *Councilors,* L. H. Adams, W. D. Collins, R. S. McBride, G. W. Morey and E. W. Washburn.

A. BRAZIER HOWELL, of the department of anatomy of the Johns Hopkins Medical School, has recently been appointed assistant news manager for the New York meeting of the American Association for the Advancement of Science. As in recent years, the association's news service is in charge of Mr. Austin H. Clark, of the U. S. National Museum.

DR. FRANK BALDWIN JEWETT, vice-president of the American Telephone and Telegraph Company, in charge of development and research; Dr. Gano Dunn, of New York City; Dr. Simon Flexner, director of the Rockefeller Institute for Medical Research; Dr. Vernon L. Kellogg, secretary of the National Research Council; Dr. Michael I. Pupin, professor of electro-mechanics in Columbia University; Dr. Max Mason, of the Rockefeller Foundation, and Dr. William Allen Pusey, of Chicago, former president of the American Medical Association, have been appointed a committee by the National Research Council, with Dr. Jewett as chairman, to advise concerning scientific matters and to cooperate in general with the officials of the Chicago World's Fair Centennial Celebration.

DR. T. WAYLAND VAUGHAN, director of the Scripps Institution of Oceanography of the University of California, has been appointed to represent the university, the Carnegie Institution of Washington and the American Association for the Advancement of Science as official delegate to the Fourth Pacific Science Congress to be held in Java from May 16 to May 23. He expects to leave in the latter part of March and to return in July.

DR. C. H. T. TOWNSEND, of Lima, Peru, formerly on the staff of the Bureau of Entomology in the National Museum as a specialist in muscoid flies, is visiting Washington and will spend about a month examining material in his specialty. Dr. Townsend has been visiting European museums for the purpose of studying types of genera in the muscoid flies.

COLONEL J. D. GRAHAM, representative of India on the health committee of the League of Nations and Office Internationale, Paris, public health commissioner with the government of India and secretary of the governing body of the Indian Research Fund Association, is visiting the United States as the guest of the Rockefeller Foundation to observe public-health activities.

DR. ELISABETH GUREWITSCH, of the University of Vienna, a pupil of Dr. M. Kohn, is spending this year at the Iowa State College with Dr. Henry Gilman on studies involving the use of the Grignard reagent. Dr. Balassa and Mr. Pfeiffer, also of the University of Vienna, have completed their studies with Dr. Gilman. Dr. Balassa is entering industrial work and Mr. Pfeiffer is returning to Austria.

With the consent of the Greek government an Italian scientific commission has gone to Greece to study dengue. Its members are: Professor Gabbi, director of the medical clinic of Parma University; Professor P. Neir, director of the Institute of Hygiene of Bari University, and Professor P. Pontano, lecturer in clinical medicine at Rome.

Dr. C. Levaditi, professor of microbiology, Institut Pasteur, Paris, will deliver the third Harvey Society Lecture at the New York Academy of Medicine, on Thursday evening, December 13. His subject will be "Metallotherapy of the Spirochaetoses."

Sir Charles A. Ballance, London, recently gave a Mayo Foundation lecture at Rochester on reminiscences of some great surgeons and neurologists of the past, with lantern slides illustrating the work of the great pathologists of the first half of the last century.

Dr. Jerome Alexander addressed the Chemistry Colloquium at the University of Michigan on November 27, his subject being "Some Principles Underlying Colloidal Dispersions, and their Bearing on a Physico-Chemical Explanation of Life and Life Processes."

Professor Arthur H. Compton, of the University of Chicago, addressed a joint meeting of the Chicago Medical Society and the Radiological Society of North America on December 5 on "Some Physical Effects of the X-Ray." Professor William T. Bovie, of Northwestern University, made an address on "The Stuff We are Made of," illustrated with lantern slides. The meeting was under the auspices of the Chicago Röntgen Society.

The Christmas week lectures on the James Mapes Dodge Foundation of The Franklin Institute, Philadelphia, will be given at three P. M. on Wednesday, Thursday and Friday, December 26, 27 and 28, in the hall of the institute. The lecturer this year will be Professor A. S. Eve, head of the department of physics of McGill University. His subject will be "Things that Spin, Things that Swing, Things that Wave."

A series of eight Lowell lectures will be given in Huntington Hall, Boston, by Vilhjalmur Stefansson on Monday and Thursday evenings, beginning on January 7 and ending on January 31. The subject is "The Northward Course of Discovery" and the aim is to show how knowledge of the Far North gradually penetrated to the lands which inherited Greek and Roman civilization. He will also give twelve lectures on "The History of Geographic Discovery" (with special reference to the Far North) at Rumford Hall, New York City, on successive evenings beginning on January 2. These are under the auspices of the New School for Social Research. A similar course, named "The Geographical History of the Arctic," will be given at Cambridge University, England, during April and May. Dr. Stefansson is also giving courses of five popular lectures each at Dartmouth, Tufts and Vassar Colleges.

On November 17, Dr. Chas. N. Gould, director of the Oklahoma Geological Survey, addressed the North Texas Geological Society at Fort Worth on "The Pennsylvanian and Permian of Oklahoma and Texas." The occasion of the meeting was for the presentation of reports by various geologists on work being done on the Pennsylvanian problem of northern Texas, this being part of the larger problem on the Pennsylvanian formations of the western interior coal fields, including the states of Iowa, Nebraska, Missouri, Kansas, Arkansas, Oklahoma and Texas, now being carried forward by the geologists of the seven states interested.

A fund to support the Priestley Lectures at the Pennsylvania State College, obtained by contributions from the alumni of the school of chemistry and physics, will be named in honor of Professor Madison M. Garver, who has been teaching at the college for thirty-five years. Professor Garver is now librarian of the chemistry library and recently celebrated his eightieth birthday. He shares the distinction of being the oldest member of the teaching staff in point of service with Professor Runkle, college historian. The Priestley Lectures deal each year with the borderline between physical chemistry and some other branch of science. The first lectures, two years ago, were given by Victor Coffman, of the Du Pont de Nemours Company. Last year Dr. S. L. Hoyt, of the research laboratory of the General Electric Company, discussed physical chemistry and metallography. In November Dr. H. B. Williams, of the department of physiology at the College of Physicians and Surgeons of Columbia University, delivered the third annual series, discussing "Method of Scientific Investigations."

At the University of Cincinnati the department of geology and geography is giving a series of illustrated popular lectures dealing with the earth sciences, as follows: December 4, Professor W. H. Bucher, "Below the Deepest Shaft—What?"; December 18, Professor O. C. von Schlichten, "Volcanoes"; January 8, Mr. C. V. Theis, "The Growth of the Ohio"; January 22, Mr. D. R. Bergsmark, "India, the Varied Land"; February 5, Professor C. H. Behre, "Our Mineral Fuels"; February 19, Professor Earl C. Case, "Some

Problems in the Development of China"; March 5, Dr. C. L. Fenton, "Trails of a Fossil Hunter"; March 19, Professor H. H. Martin, "Are Mountains an Asset?"; April 2, Professor N. M. Fenneman, "The Geology of Cincinnati." The object of these lectures is primarily to acquaint the layman with the materials of geology and geography and to raise and answer questions of popular interest in each field.

ESTABLISHMENT of an Amundsen Memorial Fellowship for furthering exploration is announced by the American-Scandinavian Foundation as a tribute to the late Captain Amundsen, the Norwegian polar explorer. The fund will remain in trust until donations have increased it to a size where the income will be sufficient to permit contributions to exploration or geographical study. It will also be used to permit deserving young men who could not otherwise pay their own expenses to join important expeditions.

FRANCIS P. LEAVENWORTH, professor emeritus of astronomy at the University of Minnesota, died on November 12, at the age of sixty-nine years.

THE death of Professor George I. Kemmerer, of the department of chemistry of the University of Wisconsin, is announced. Dr. Kemmerer's death at the age of forty-nine years was sudden and unexpected.

STRICKLAND LANDIS KNEASS, vice-president of the engineering firm of William Sellers and Company, died on November 25 at the age of sixty-seven years. Mr. Kneass was known for an injector which he devised for locomotive boilers and for his work on the discharge of elastic fluids and the flow of steam.

SYDNEY ROYSTON PIKE, research fellow of the International Education Board at the Mount Wilson Observatory, died on November 22 at the age of twenty-five years. Before going to Pasadena Mr. Pike had been for three years assistant lecturer at the University of Leeds, where he had carried on research work on the solar atmosphere, which was published in the *Notices* of the Royal Astronomical Society.

A CORRESPONDENT writes: "Announcement is made of the death of William Theodore Gauss on November 14, at his home in Colorado Springs, Colorado, at the age of seventy-seven years. He had been ill for two weeks. Mr. Gauss was a grandson of the illustrious German mathematician, Carl Friedrich Gauss, and through his mother was a nephew of the noted German astronomer, Friedrich Wilhelm Bessel. For many years he spent much time and money in assembling a large and valuable collection of Gauss memorabilia, which is being now used by G. Waldo Dunnington, of Washington and Lee University, in a biography of C. F. Gauss. This death reduces the number of Gauss's grandchildren to four, one of whom resides in California, and the remainder in Missouri. His brother, the Reverend Dr. Joseph H. Gauss, is superintendent of the Brookes Bible Institute in St. Louis, Missouri. He was born in Chariton County, Missouri, on July 1, 1851, and engaged in the wholesale shoe manufacturing business in Boston and St. Louis. He later moved to Colorado Springs where he was a broker and mining stock promoter. Mr. Gauss was conspicuous for his devotion to his family and home. His qualities of character and his profound sense of honor, with his unselfish nature, made him many friends throughout his life."

MISS EVELYN M. GRAY, editorial assistant of *Genetics* at the Connecticut Agricultural Experiment Station, died on November 14.

THE *Journal* of the American Mathematical Society states that by the will of Lieutenant-Colonel A. J. C. Cunningham, who died February 8, 1928, the London Mathematical Society will receive his library and also £1,000 for the improvement of methods of factorization of large numbers, and £2,000 for the publication of Colonel Cunningham's works. One twelfth of the residuary estat. also will go to the London Mathematical Society, and one twelfth to the mathematical subsection of the British Association, for preparing new mathematical tables in the theory of numbers.

UNIVERSITY AND EDUCATIONAL NOTES

THE University of the City of Toledo was successful in its recent campaign for a $2,850,000 bond issue for new buildings. Of this amount, $300,000 will be expended for land and $2,550,000 for buildings and equipment. In addition the university possesses land and buildings valued at more than $1,000,000 and this property will ultimately be sold and the proceeds used for further construction. A committee of the board is now investigating proposed sites, and the erection of buildings will probably begin in January.

DARTMOUTH COLLEGE has received a gift of $100,000 from Mrs. William P. Johnson in fulfilment of a promise made before his death by Mr. Johnson.

THE new library of the Philadelphia College of Pharmacy and Science was dedicated on November 26 with addresses by Mr. John B. Raser, of the class of 1871; Dr. Wilmer Krusen, president, and Dr. Charles H. La Wall, dean of pharmacy. The library equipment was a gift to the college by Mr. Raser and his brother, Mr. William H. Raser, of New York, a grad-

uate of the class of 1868, and Mr. Raser's son, Mr. William Heyl Raser, of Reading, Pennsylvania, a graduate of the class of 1901. The library, which is one of the largest and most complete pharmaceutical libraries in the United States, includes the extensive private library of scientific books formerly owned by the late Professor Joseph P. Remington.

A GRANT has been made by the Julius Rosenwald Fund of $80,000 to be expended over a period of five years to the department of zoology of Howard University, to make possible the continuation of the research of Dr. Ernest E. Just and to build up a department offering instruction to graduate students.

DR. JOHN ROSCOE TURNER, formerly dean of the college of arts and science at New York University, was on November 28 installed as president of the West Virginia University. Among the representatives of the one hundred and forty-two American colleges and universities present at this occasion may be mentioned Dr. Samuel W. Parr, president of the American Chemical Society; Professor Paul M. Lincoln, head of the electrical engineering department of Cornell University, and Dr. David White, senior geologist of the U. S. Geological Survey, who addressed the science and engineering group; Dr. Thomas Peck Sprunt, associate in medicine at the Johns Hopkins University, and Dr. Harry M. Hall, president-elect of the West Virginia Medical Association, who addressed the medical group; Governor Howard M. Gore, governor of the State of West Virginia, and Dr. Albert F. Woods, director of scientific work in the U. S. Department of Agriculture, who addressed the agricultural group. Addresses were also made by noted jurists and educators before the law and education groups. The LL.D. degree was conferred upon Dr. Turner prior to his installation.

DR. HENRY DASPIT has been appointed dean of the graduate school of medicine of Tulane University of Louisiana, New Orleans, succeeding Dr. Edmund D. Martin.

THE following promotions on the Stanford faculty were made recently: Associate professors (to be professors): Charles Moser, in civil engineering; Clelia D. Mosher, in personal hygiene for women. Assistant professors (to be associate professors for a five-year term): Claus W. Jungeblut, in bacteriology; James Percy Baumberger, in physiology.

DR. E. J. LORENZ, of the California Institute of Technology, has been appointed professor of physics in the University of the City of Toledo.

EDGAR W. WOOLARD, assistant meteorologist, U. S. Weather Bureau, has resigned to accept an appointment as instructor in the department of mathematics at George Washington University.

FREDERICK JOHN MARRIAN STRATTON, fellow and tutor of Gonville and Caius College, formerly assistant director of the solar physics observatory, has been elected to the professorship of astrophysics at the University of Cambridge.

DISCUSSION AND CORRESPONDENCE

SAVING THE COCONUTS

WHEN recently in the Fiji Islands, I was able to see something of the important work in economic entomology carried on there. At Suva I found a Department of Agriculture, with Dr. J. D. Tothill in charge. I saw the details of the work and got much information from Messrs. R. W. Paine and H. W. Simmons. At Levuka I found Mr. and Mrs. T. H. Taylor, who had returned from an expedition to Trinidad (British West Indies), bringing five species of Coccinellid beetles (the best being *Cryptognatha nodiceps* Marshall) to prey on the scale insect *Aspidiotus destructor translucens* (Ckll.) (*A. transparens* of Green 1899, not 1890) which is so injurious to the foliage of coconut in Fiji. Mr. Taylor said that the scale was the third in importance of the coconut pests in Fiji. The first (until lately) being *Levuana*, discussed below; the second *Tirathaba*, a Pyralid moth with the aspect of a noctuid, the larva boring in the spathe. At Lautoka I found Mr. H. Greenwood, from New South Wales, in charge of the entomological laboratory of the Colonial Sugar Refining Company. He is a keen botanist as well as entomologist, and has been making a catalogue of all the insects of the Fiji Islands. I was able to witness the work against the sugar-cane weevil, *Rhabdocnemis obscurus* (Boisduval), by means of the dipterous parasite *Ceromasia sphenophori* Villen. This I hope to describe later, in a discussion of the pests of the sugar cane.

The matter of the greatest and most dramatic interest is the conquest of *Levuana* by an introduced dipterous (Tachinid) parasite. *Levuana iridescens*, a small, dark-colored, inconspicuous moth, was described by Bethune-Baker, a well-known English lepidopterist, in 1906. His material was from the Fiji Islands, and the genus *Levuana* contains to this day only the species *L. iridescens*. It belongs to the family Zygaenidae, known in the United States especially by those little caterpillars which appear in rows, like well-drilled soldiers, on the leaves of grape vines. The rather slug-like caterpillar of *Levuana* eats the green tissue of the coconut leaves, and when sufficiently numerous will kill the tree.

There is an old record of a coconut pest believed to be *Levuana,* in Fiji as early as 1871, but there is reason to believe that the species was introduced from some as yet undiscovered locality. Mr. Taylor told me that in New Guinea he found many related species feeding just as *Levuana* does, on palms and plants of the ginger family (Zingiberaceae). He did not, however, find *Levuana.* In New Caledonia I saw no trace of *Levuana,* though the coconuts were attacked by other pests, which I hope to discuss at another time.

Levuana remained in Fiji, attracting little attention, because not yet wide spread, for a number of years. But eventually it came to be recognized as a major pest, and one which, unchecked, would destroy all the coconuts. It began to spread, and in 1921–22 reached the Island of Ovalau. The indications were that it would eventually spread all over the Pacific Islands, destroying the coconut and bringing calamity to the natives. Mr. Paine told me that it reached Malolo, off the west coast, and killed the coconuts, itself perishing apparently from lack of food. It is not, however, confined to the coconut; it will attack ornamental palms and has been seen feeding on the leaves of the banana. So abundant did *Levuana* become that many perished because the cocoons were spun in masses, one over the other, with the result that the moths hatching below died without being able to emerge.

No enemies were adequate to check the scourge. A large attid spider of the subfamily Cytaeinae (*Ascyltus pterygodes* L. Koch) attacks *Levuana.* It is a beautiful animal, the cephalothorax and abdomen above green, the legs partly purple. Three species of Pentatomid bugs are enemies of *Levuana.* One, *Oechalia consocialis* (described from Australia), is a narrow species, with pointed scutellum. The second, *Cantheconidea cyanacantha* (described from Fiji), is brown, obtuse, the scutellum with a metallic blue spot on each side at base, the lateral spines of the prothorax also blue, as the specific name indicates. The third, *Platynopus* or *Pinthaeus melanacanthus,* has long, blue-black spines at sides of thorax, and has the end of the scutellum broad and pale yellow. A clerid beetle, *Callimerus arcufer,* was imported from the Malay Peninsula to feed on *Levuana,* but it was of no use, and the many colonies liberated apparently failed to establish themselves. The outlook was thus, for a time, discouraging, as none of the enemies mentioned could abate the plague.

In 1925 Dr. Aldrich, of the U. S. National Museum, described a new fly of the family Tachinidae as *Ptychomyia remota.* It was sent from the Federated Malay States, with the statement that it was a parasite of *Artona catoxantha* Hampson, a zygaenid moth seriously attacking coconuts. Dr. Tothill and his associates obtained living material of this fly, and liberated it in Fiji. Results were apparent in about six months. The fly spread with great rapidity, and in a short time the *Levuana* pest was practically a thing of the past. When I was in Suva, I was shown the coconut palms, with fresh green foliage, ready to bear abundant fruit. I could only imagine from descriptions what they looked like not long ago.

The entomological work in Fiji is supported to the extent of about half by the government, the other half by the planters, through a tax on copra.

As an example of successful biological control, through cooperation among scientific men, the case of *Levuana* could hardly be excelled. It should encourage further efforts along similar lines, teaching at the same time the importance of patiently and industriously following every clue. The only disadvantage is that a section of the public, seeing the entomological magicians thus work miracles, do not understand why they can not control any pest at any time.

T. D. A. COCKERELL

"AN UNEXPLAINED VISUAL PHENOMENON"

UNDER the above title, in SCIENCE for October 26, Mr. Gradle reported an extremely interesting observation. I am wondering whether the phenomenon is not due to the same underlying cause as the occasional stationary appearance of a rotating spoked wheel. Also, I have frequently observed that when I was stropping a razor, it gave the jerky appearance familiar in the "movie" of a rapid action; this appearance has occurred in sunlight as well as in artificial light. I have questioned several psychologists about it without any result. My only explanation (purely a hypothesis!) is that in certain individuals vision is not a steady process, but occurs at regular intervals, as by a stroboscopic disk. Or shall we say that vision is also quantized? Mr. Gradle can easily test this in his case by adjusting the speed of a spoked wheel until he can repeat his observation on the propeller.

As to the position of his eyes, it is well known that the peripheral portions of the retina have more acute vision in dim light than the central portion. When I walk over a trestle in dim light I always look straight ahead, and can see the ties more easily than when looking down.

PAUL F. GAEHR

WELLS COLLEGE,
AURORA, NEW YORK

REFERRING to Mr. Harry S. Gradle's letter in SCIENCE,[1] I might say that I have the same sort of

[1] October 26, 1928, p. 404.

experience every day. If I look at the wheels of an automobile which is passing me at moderate speed, perhaps of ten to fifteen miles per hour, they will seem to stop stock still about once per second while the car is within 45 degrees of my straight-ahead direction. The phenomenon is much clearer with wooden spokes than with wire ones. I can see all the wooden spokes perfectly distinctly.

I have never been able to get an oculist interested enough in the thing *facere aliquid experimentum in corpore vili,* and I have never taken the time to experiment myself with a rotating disk. I have been inclined to attribute the phenomenon to retinal fatigue.

There is a peculiarity in my vision which may have something to do with it. Presbyopia has hardened my lenses asymmetrically, and each eye has one fairly distinct false focus and several quite indistinct ones. At the proper distance from an electric sign at night I can read about half the letters at the false focus. With the reverse illumination, as with black print on white paper, it doesn't bother me, as the spurious images merge in the white background and are too faint to see. On a dark day, when my pupils are wide open, I can just see them. The trouble is in the periphery of the lenses. A two or three millimeter pinhole obviates most of the trouble.

R. M. PACKARD

BOSTON, MASSACHUSETTS

NOTE UPON THE OCCURRENCE OF OTOMESOSTOMA AUDITIVUM (PLESS.) IN THE UNITED STATES

IN Ward and Whipple's "Fresh-water Biology" (18) the statement occurs that no fresh-water representative of the suborder *Alloeocoela* has been definitely established for the United States (page 354). Higley (18) records new species of rhabdocoeles for the Mississippi Valley but does not mention any genus or species of the suborder *Alloeocoela.* Nor have I been able to find any reference to this suborder which would indicate that it occurred in the United States. I would like to record, therefore, that I have found and definitely identified *Otomesostoma auditivum* in the streams and pools near the University of Virginia. I have found only five specimens so far, three in April, one in May and one in July. Of those I found in April two were sexually mature and produced eggs which later developed. The young ate heartily of tadpole's brain and were developing in fine fashion until the culture was accidentally destroyed.

I have found specimens of two other species which, from the general characteristics ascribed to this suborder by von Graff, I am convinced belong to the suborder *Alloeocoela.* These species have not been identified definitely as yet.

It seems, from these instances, that members of this suborder do exist in the United States, and that by careful observation and study they will be found to be somewhat abundant.

J. S. CARTER

UNIVERSITY OF VIRGINIA

SALMONELLA COLUMBENSIS

SALMONELLA COLUMBENSIS was first described by Castellani in 1905 under the name of *Bacterium columbense.* He isolated it from cases clinically similar to typhoid fever of medium severity. The microorganism was motile, Gram negative and did not produce spores. It produced acid and gas in dextrose, maltose, dulcite, mannite, dextrin (slight amount), arabinose, sorbite, galactose, levulose, salicin, rhamnose and gylcerine no acid or gas was produced in saccharose, raffinose, adonite, inulin, inosite or amygdalin. Acid and gas production in lactose was variable. It liquefied neither gelatin nor serum. Tests with typhoid serum, paratyphoid A serum and paratyphoid B serum, were distinctly negative.

The writer recently isolated a microorganism similar to the above from the stool of a non-febrile patient. This microorganism did not produce acid or gas from either lactose or dextrin. In addition to the carbohydrates used by Castellani, acid and gas were produced in xylose and trehalose. There was no agglutination with typhoid, paratyphoid A or paratyphoid B serums. Agglutination with *Salmonella columbensis* serum was complete in full titre.

Judging from the available literature, it would appear that *Salmonella columbensis* infections and carriers are quite rare in this country, there being no report in the literature reviewed.

FREDERICK W. SHAW

MEDICAL COLLEGE OF VIRGINIA

INHALATION THE CHIEF FACTOR IN ONION OR GARLIC CONTAMINATION OF MILK

IN sections where wild onion and garlic flourish agricultural experiment stations, dairymen and consumers of dairy products have long been interested in measures to prevent contamination of the milk of cows on pasture during the spring and fall seasons of growth of these weeds. The desirability of accomplishing the result by supplementary feeding has suggested many experiments which have failed to solve the problem and has encouraged the sale of numerous proprietary feeds which have not fulfilled their guarantees.

Due to the volatile nature of the essential oils of onion and garlic, the constituents may enter the blood from the respiratory as well as from the digestive tract. A rapid transfer of these substances from the blood to the fat of the milk takes place in the udder. Ordinary ingestion of the plants permits the acquisition of the onion or garlic substances by the blood through both channels.

In recent experiments at the University of Tennessee, the administration of fresh wild garlic tops was limited to inhalation of the volatile substances. Under these conditions, strong garlic flavor and odor were detected in the blood and milk in a very short time. When, however, the administration was so arranged as to eliminate as far as possible the inhalation factor, the time required for the acquisition of the garlic flavor and odor by the milk was greatly increased and the intensity markedly diminished.

It is evident from the results obtained in these experiments that of the two modes of entrance of garlic into the blood, inhalation is many times more effective. Under ordinary conditions this factor can not be eliminated nor can it be controlled by feeding.

MARGARET B. MACDONALD
M. JACOB
AGRICULTURAL EXPERIMENT STATION,
KNOXVILLE, TENN.

QUOTATIONS

THE CONTROL OF MALARIA

AN important development in the control of malaria is announced by the Ross Institute and Hospital for Tropical Diseases. A new organization of the Ross Institute Industrial Anti-Malarial Advisory Committee has been set up to assist tropical industries in combating the disease. One of the technical members of the committee will be Sir Malcolm Watson, who, after many years of active malarial control work in the Malay States, has joined the Ross Institute as principal of its malarial control department. The others will be the three directors of the institute—Sir Ronald Ross, Sir William Simpson and Sir Aldo Castellani. The lay members of the committee will represent the Indian Tea Association, the Rubber Growers' Association and other Indian and African iron, mining, jute and cotton companies. The anti-malarial staff at the institute will be at the disposal of these companies for advice, and will study their medical reports and receive and advise their physicians. It is intended that Sir Malcolm Watson himself shall visit some part of the tropics every year for a short period. The past twenty-eight years' work in Malaya has shown that research has been benefited so much by malaria control and the work so much from research that it is considered advisable to associate research and active malaria control work as closely as possible in the future. The study of malaria in Malaya has shown how it varies with the physical features of the land and the species of anopheles inhabiting it. Even in a country so small as the Malay Peninsula malaria is associated with various zones of land (*e.g.*, salt water, mangrove swamps, fresh-water coastal swamps, the ravines of coastal hills, the inland plains, the inland hills) in certain definite ways, depending on whether or not the virgin jungle is intact or destroyed. That gave ten types of land which had to be studied. In addition, rice fields of three types were discovered, giving a total of thirteen different types of land. Some twenty-one different species of anopheles had to be classified in the laboratory and studied in the fields. It was found that probably only four or five were associated with the spread of malaria on any considerable scale and therefore expenditure on the control of others was unnecessary. The methods devised for the control of malaria had to vary widely; in some places there was strict preservation of jungle, and in others elaborate engineering schemes, the application of larvicides, drainage or flooding. In India, malaria is associated with other types of land, *e.g.*, the tanks surrounding villages in the great plains, the rivers and hills of the Assam valleys, and the wells in large cities. Enormous sums of money have been saved to the governments and industrial undertakings in Malaya by the control of malaria. It is certain that there will be an increasing demand for control by both the governments and industrial undertakings in other countries. Without trained research medical officers and expert engineers, the attempt to control malaria will be unduly expensive, and in many cases will fail. The lesson was a dearly bought one in Malaya. The Ross Institute Industrial Anti-Malarial Advisory Committee, with local branches in various countries, is Sir Ronald Ross's solution of this problem.—*The Journal of the American Medical Association.*

PROTOCHLOROPHYLL

PRINGSHEIM[1] assumed alcohol extracts of etiolated seedlings to have a yellow pigment which he called *etiolin*. The spectrum of etiolin showed in addition to the absorption bands characteristic of chlorophyll, a band between $\lambda = 640$ and 620 $\mu\mu$. Monteverde[2] made an exhaustive study of alcohol extracts of etiolated seedlings and found the absorption band observed by

[1] Monatsber. d. kgl. Akad. d. Wiss. zu Berlin, Okt. 1847, Sep. p. 6.

[2] Acta Horti Petropolitani 13, Nr. 11, p. 210, 1894.

Pringsheim to be due to a peculiar red-fluorescing pigment, usually masked by the yellow pigments, which he called *protochlorophyll*. Liro[3] observed that absolutely dark-grown seedlings do not have the etiolin-spectrum of Pringsheim unless they are exposed to the light for a few seconds or minutes, and rightly concluded that etiolin is a mixture of the protochlorophyll of Monteverde and chlorophyll.

Liro made an extensive study of the formation of chlorophyll in the phanerogams and concluded that protochlorophyll is a postmortal decomposition product of a colorless organic substance which develops in the dark, and is designated, after Sachs,[4] as *leucophyll*. According to Liro, leucophyll decomposes to form protochlorophyll in cells that are killed, and changes photochemically into chlorophyll upon exposure to light. Protochlorophyll may be observed in living leaves, but Liro assumed this to be due to occasional dead cells in which the leucophyll has undergone decomposition. In support of this view Liro reports that a layer of five or more living leaves of etiolated seedlings of *Avena sativa, Triticum sativum* or *Sinapis alba* was necessary, when examined spectroscopically, for the detection of the absorption band characteristic of protochlorophyll, but after the leaves were killed by dipping them in boiling water a single leaf or a layer of only two leaves was sufficient to give an evident protochlorophyll spectrum. It is assumed that in killing the cells the protochlorophyll is formed as a decomposition product of leucophyll. This conclusion is contradicted by practically all the experiments in Liro's paper, as he claims that leucophyll changes quantitatively into chlorophyll in etiolated leaves upon exposure to light even though the cells have been killed in any manner whatsoever, as by freezing, by immersing in boiling water, by exposing to ether vapors, by drying, by grinding to a paste in a mortar. Leucophyll has never been extracted from plants, though Liro tried many substances as solvents. His conclusion is that the leucophyll was either destroyed by or was insoluble in each of the substances tried.

The presence in plants of leucophyll, in the sense of Liro, is entirely hypothetical. That protochlorophyll is a decomposition product of some other organic substance seems highly improbable in the light of recent studies on the formation of chlorophyll made in the Botanical Institute of the University of Erlangen, Germany. It has been found that absolutely dark-grown seedlings of *Zea Mays* and *Avena sativa* contain only protochlorophyll when extracted in the dark, protochlorophyll and chlorophyll when extracted in red light in the dark room, and only chlorophyll when extracted in diffuse daylight in the laboratory. The relation of protochlorophyll to chlorophyll is always quantitative, and as the amount of protochlorophyll decreases under the influence of light the amount of chlorophyll increases. Protochlorophyll is changed into chlorophyll by red light which has no photochemical effect on orthochromatic photographic emulsions. As found by Liro, red light is about twenty times as effective as blue light in converting protochlorophyll (leucophyll, according to Liro) into chlorophyll. In daylight protochlorophyll changes quantitatively into chlorophyll very rapidly. Etiolated seedlings of *Avena sativa,* which had been air-dried in the dark-room and stored in a covered box in the laboratory for approximately one year, were extracted in the light and were found to contain protochlorophyll and magnesium-free protochlorophyll, but no chlorophyll. Protochlorophyll in ether solution does not change to chlorophyll when exposed to light.

Dr. Noack, of the Botanical Institute of the University of Erlangen, has made preparations of pure protochlorophyll, and established its chemical relationship with a similar pigment which occurs in the gall of animals.

From the results of the studies briefly referred to above, protochlorophyll is not a decomposition product of some other organic substance, as leucophyll, but is a pigment which develops without the influence of light and changes photochemically into chlorophyll upon exposure to light. It is probable that this change occurs only in the presence of a specific enzyme.

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[3] Annales Academiae Scientiarum Fennicae, Ser. A, Tom. 1. 1909.

[4] *Lotos*, 1859.

INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE OPINIONS 98 TO 104

The undersigned has the honor to invite the attention of the zoological profession to the fact that Opinions 98 to 104 have been published by the Smithsonian Institution.[1] The summaries read as follows:

Opinion 98. Rigidly construed, Brauer and Bergenstamm (1889 to 1894) did not fix the types for the older generic names, except in the cases where they distinctly state that the species mentioned is the type of the genus.

[1] *Smithsonian Miscellaneous Collections*, 73; No. 5, pp. 1–28.

Opinion 99. *Entamoeba* 1895, with *blattae* as type by subsequent (1912) designation, is absolute synonym of *Endamoeba* Leidy, 1879a, p. 300, type *blattae*, and invalidates *Entamoeba* 1895, type by subsequent (1913) designation *hominis* = *coli*.

Opinion 100. Under Suspension of the Rules the genotype of *Spirifer* Sowerby, 1816, is fixed as *Anomia striata* Martin, and the genotype of *Syringothyris* Winchell, 1863, is fixed as *Syringothyris typa* Winchell (= *Spirifer carteri* Hall).

Opinion 101. The technical Latin designations used by Danilewsky, 1891, Annales de l'Institut Pasteur, Vol. 5 (12), pp. 758–782, are not in harmony with the International Rules of Zoological Nomenclature and are therefore not subject to citation or the Law of Priority on basis of said publication.

Opinion 102. A generic name (example *Proteocephalus*, 1858) is not invalidated by the earlier publication of the identical or a similar name of higher rank (example *Proteocephala*, 1828). If *Taenia ambigua* (tod. of *Proteocephalus*, 1858) is congeneric with *ocellata* (tsd. of *Ichthyotaenia*, 1894), *Ichthyotaenia* is a subjective synonym of *Proteocephalus*.

Opinion 103. The type of *Grus* Pallas, 1767, is *Ardea grus* Linn., 1758, by absolute tautonymy. *Grus* is hereby placed in the Official List of Generic Names.

Opinion 104. The following 57 generic names, with type species cited, are hereby placed in the Official List of Generic Names:

PROTOZOA: *Bursaria, Eimeria, Laverania, Plasmodium, Sarcocystis.* CESTODA: *Ligula.* NEMATODA: *Filaria, Heterodera, Rhabditis, Strongylus, Syngamus.* OLIGOCHAETA: *Enchytraeus.* HIRUDINEA: *Haemadipsa, Limnatis.* CRUSTACEA: *Armadillidium, Astacus, Cancer, Diaptomus, Gammarus, Homarus, Nephrops, Oniscus, Pandalus, Penaeus, Porcellio.* XIPHOSURA: *Limulus.* SCORPIONIDEA: *Scorpio.* ARANEAE seu ARANEIDA: *Avicularia, Dendryphantes, Dysdera, Latrodectus, Segestria.* ACARINA: *Cheyletus, Chorioptes, Demodex, Dermanyssus, Glyciphagus, Polydesmus, Psoroptes, Rhizoglyphus, Trombidium.* THYSANURA: *Lepisma.* COLLEMBOLA: *Podura.* ORTHOPTERA: *Blatta, Ectobius, Gryllus, Periplaneta.* ANOPLURA: *Pediculus, Phthirus.* HEMIPTERA: *Anthocoris, Nabis, Notonecta, Reduvius, Triatoma.* DERMAPTERA: *Forficula.* SUCTORIA s. SIPHONAPTERA s. APHANIPTERA: *Pulex.* MAMMALIA: *Cercopithecus.*

C. W. STILES

Secretary to the International Commission on Zoological Nomenclature

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE SUBCUTANEOUS LYMPH SAC OF THE FROG AS A CULTURE CHAMBER

MUCH of the tedium involved in maintaining tissue cultures may be avoided by taking advantage of the natural culture chamber afforded by the subcutaneous lymph sacs of the frog. The large ventral lymph sac is especially suitable for this purpose. A slit cut in the ventral skin in the region of the sternum makes it possible to slip excised bits of various frog tissues into the ventral lymph sac, where conditions are favorable for a continuation of living processes in the explanted tissue. The lymph of the living host serves as an aseptic and nutritive medium for the explant, and the explanted tissue may be left undisturbed until the conclusion of the experiment.

Lymph-sac cultures of integument may be maintained for two months or more, but the differentiated cells of liver and kidney undergo early disintegration. Explants of stomach wall and lung wall are more often successful in the lymph sac than those of liver or kidney but do not persist as long as do cells of integument.

In cases where small pieces of integument were inserted into the ventral lymph sac with their epidermis in contact with the subcutaneous surface of the skin of the host, sections taken from a series of such cultures show that the epidermal cells move out from the cut edges of an explant and migrate along the surface of a lymph coagulum which forms between the explant and the subcutaneous surface of the ventral skin of the host. At about forty-eight hours after operation, the migrating cells complete the formation of a vesicle, the wall of which consists, in part, of a newly formed epithelial layer and, in part, of the original explant. The tendency of pieces of integument to form vesicles when cultivated in the frog lymph sac was reported by Winkler (1910), who made no attempt to account for the phenomenon.

About thirty days after operation, sections show that growth of the dermal cells of the explant has begun. This dermal growth continues until the vesicle mentioned above becomes completely invested with dermis. Beginning about three weeks after operation, the subcutaneous blood vessels of the host skin produce branches which invade the newly formed dermis of the vesicle. In later stages up to fifty-five days, these invading blood vessels are filled with normal red blood corpuscles, which fact indicates that these vessels are active in connecting the vesicle with the cutaneous circulation of the host. How long such a parasitic existence will persist can be determined only by further experimentation.

Cultures of frog integument in the lymph sac are equally successful, whether the explanted tissues and the host are of the same or of different species of frog. At least this is true in cases where the two species used are *Rana pipiens* and *Rana clamitans*.

This method of tissue culture fails to afford opportunities for direct observations upon living cells, but

it eliminates many of the difficulties attendant upon experiments involving hanging drop cultures.

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A MICRO-TECHNIQUE FOR OBSERVING OIL PENETRATION IN CITRUS LEAVES AFTER SPRAYING

THE rapidly increasing use of white oil of high viscosity in spraying for the control of scale pests of citrus has been followed in many instances by injurious effects, such as fruit drop, leaf drop, dead-wood, reduction of bloom and reduction of crop, etc.; hence the need for a careful study, by means of the microscope, of the penetration and disposition of oil within the plant tissue.

The problem presented several difficulties. The exigencies of the case precluded the employment of the ordinary solvents used in histological technique. It was necessary to fix or clear the specimen without disturbing or dissolving the oil. Consequently, the use of alcohol, xylol or any other of the essential oils was prohibited, as was also the paraffin method of imbedding which necessitates infiltration with chloroform or ether.

It was desirable to examine both flat sections, cleared and stained, and also fixed cross-sections. This has been accomplished by the use of an aqueous solution of pyridin to dissolve chlorophyll, and Oil Red O[1] dissolved in aqueous-pyridin as a stain.

The technique is substantially as follows:

For Flat Sections:

Immerse specimen in a 60 per cent. aqueous solution of pyridin (60 parts pyridin to 40 parts distilled water).

Heat over water bath. When discolored pour off and refill. Repeat till solution remains clear and specimen becomes transparent. (This can be conveniently done with the use of a small shell vial inserted through the center of a flat cork and floated on water bath.)

Immerse in saturated solution of Oil Red O in 70 per cent. pyridin (70 parts pyridin to 30 parts distilled water) for 24 hours.

Differentiate in 50 per cent. pyridin, until stain ceases to stream. (This takes but a few minutes.)

Wash in running water one hour or more.

Pass through, first glycerine water (equal parts). Second glycerine.

Clear in carbol-glycerine (1 part carbolic acid to 2 parts glycerine). Heat gently in watch glass and observe carefully under dissecting microscope till clear. Specimen should be turned under side up, when oil drops can be distinctly seen.

Pass through glycerine again. (This is important and prevents clouding on the slide.)

Mount in glycerine jelly. Allow to harden and seal with clear Duco.

For Cross-Sections:

Fix in chrome-acetic acid 48 hours.

Wash in running water.

Immerse in 5 per cent. formalin 1 hour.

Wash in running water.

Immerse in 50 per cent. pyridin 10 minutes.

Stain in saturated solution of Oil Red O in 70 per cent. pyridin 24 hours.

Differentiate in 50 per cent. pyridin until color ceases to stream. (Watch carefully.)

Wash in running water.

Section in pith. (The freezing method might be used to advantage but has not been tried.)

Pass through, first glycerine water (equal parts). Second glycerine.

Mount in glycerine jelly and seal with clear Duco.

The oil stains a bright orange to deep red, depending upon the length of time allowed for the staining process. Heavy oils take longer to stain than light. Essential oils, lipoids and other fatty bodies, as well as cutin, also stain but are readily distinguished from the oil. Essential oils are confined to certain well-defined oil cells; they do not occupy the intercellular spaces. Lipoids and other fatty bodies stain deep scarlet, almost black. Cutin stains yellow and, if oil soaked, orange to red.

By employing this technique the writer has been able to observe the penetration of oil into the leaf, its translocation through the vascular system into the stem and across the medullary rays to its final deposition in the storage cells of the pith and old wood fibers. Oils of high viscosity choke the vascular system, to a greater or less degree depending upon the amount of oil, for an indefinite period of time.

HUGH KNIGHT

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[1] F. Proescher, "Oil Red O a Rapid Fat Stain," *Stain Technology*, Vol. 2: 60.

SPECIAL ARTICLES

ON THE STRUCTURE OF THYMONUCLEIC ACID

THE plant nucleic acid is regarded as a tetranucleotide, each nucleotide being composed of phosphoric acid, a sugar (ribose) and a nitrogenous component. The evidence for this theory of structure is complete, inasmuch as it was possible to decompose the nucleic acid into the individual nucleotides, and each of the nucleotides into phosphoric acid and the complex consisting of the sugar and a base.

For the thymonucleic acid an analogous structure was suggested. The evidence, however, was incomplete, since it was impossible to decompose by chemical means the thymonucleic acid in such a manner

as to obtain the complexes consisting of the sugar and bases. Such complexes are known by the name "nucleosides."

In the development of the theory of structure of the plant nucleic acid the nucleosides played a very important part inasmuch as they made it possible, on one hand, to explain the order of union between the individual components of the nucleotides, and, on the other, they afforded a way of isolating in pure state the sugar entering in the structure of the plant nucleic acid. It is evident that the isolation of the nucleosides from the thymonucleic acid will play an analogous rôle in the development of the theory of the structure of thymonucleic acid.

One of these nucleosides has now been isolated in perfectly crystalline form, free from mineral impurities. It is optically active; on hydrolysis it gives rise to a reducing substance and to the base guanine. The reducing substance does not form an osazone under the usual conditions. With Kiliani's reagent the substance gives a greenish-blue coloration which on standing turns to purple. The color is not identical with that described by Kiliani for his desoxysugar. The composition of the nucleoside, however, suggests the possibility of the sugar being either an anhydro- or a desoxyhexose. The theory of the guanine nucleoside of an anhydrohexose requires the following composition: C = 44.74, H = 4.41, N = 23.73. The analytical results found for our substance are C = 44.43, H = 4.41, N = 24.65. The slight discrepancies between the theoretical and the found values are easily explained by a slight admixture of the free base, inasmuch as it is known that nucleosides have a tendency to form complex salts with the free bases.

We request workers in the field of nucleic acids to leave for some time to come further work on this substance to our laboratory.

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E. J. LONDON

THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH, NEW YORK
November 28, 1928

*THE SPECTRUM OF IONIZED XENON (Xe_{II})

EXTRAPOLATING the separation of the lowest doublet ($^2P'_{2,1}$) of the spark spectra of the preceding rare gases, arising from the s^2p^5 electron configuration, has made possible the estimation of this separation in the first spark spectrum of xenon. The data of Abbink and Dorgelo[1] in the Schumann region have been examined and the doublet separation has been found to be 9621 wave-number units. The most probable classification of the combinations in this region of the spectrum is as follows:

Combination	λ	ν
$(s^2p^5)\,^2P_2' - (s^2p^4\cdot 4s)\,^2P_1$	824.83	121237
$(s^2p^5)\,^2P_2' - (s^2p^4\cdot 4s)\,^2P_2$	854.71	116999
$(s^2p^5)\,^2P_1' - (s^2p^4\cdot 4s)\,^2P_1$	895.92	111617
$(s^2p^5)\,^2P_1' - (s^2p^4\cdot 4s)\,^2P_2$	931.25	107383
$(s^2p^5)\,^2P_2' - (s^2p^4\cdot 4s)\,^4P_1$	1003.36	99665
$(s^2p^5)\,^2P_2' - (s^2p^4\cdot 4s)\,^4P_2$	1051.93	95063
$(s^2p^5)\,^2P_2' - (s^2p^4\cdot 4s)\,^4P_3$	1100.46	90841
$(s^2p^5)\,^2P_1' - (s^2p^4\cdot 4s)\,^4P_1$	1110.62	90039
$(s^2p^5)\,^2P_1' - (s^2p^4\cdot 4s)\,^4P_2$	1170.43	85439

There is some evidence of other combinations both in this region and in the visible spectrum. The interval between the terms tentatively named ^4P and ^2P is abnormally large, and, although the identification of the former is fairly certain, the final interpretation of the latter is reserved until further data are available. Work is now in progress on a complete description and classification of the spectra of xenon. The authors are expecting to make a further report as soon as the investigation can be completed.

C. J. HUMPHREYS
T. L. DE BRUIN

THYROID-FED RATS AND HIGH ROOM TEMPERATURES[1]

DURING the progress of some experimental work on the effects of feeding desiccated thyroid glands upon the reproduction and lactation of the white rat, the results of which will be published in another journal, one experiment was terminated suddenly when all the thyroid-fed rats died as the result of room temperatures of 88° and 92° F.

A search through the available literature showed that Korenchevsky[2] had reported that "after long excessive thyroid feeding, warming may even be followed by a lethal overheating with a rise of body temperature to 43.5° C."

In view of the fact that 92° F. (33° C.) is considerably less than Korenchevsky's lethal temperature of 43.5° C., it was thought that it would be worth while to publish the following report at the present time.

We had found that, if the dosage of desiccated thyroid was increased a definite amount each time a female rat gained twenty-five grams in body weight, many of her litters were born dead and normal lactation was so interfered with that many or all

* Publication approved by the Director of the Bureau of Standards of the U. S. Department of Commerce.

[1] J. H. Abbink and H. B. Dorgelo, *Zeits. f. Physik*, 47, 221 (1928).

[1] This research was aided in part by a grant from the Committee on Problems of Sex of the National Research Council.

[2] V. Korenchevsky, *Journ. Path. and Bact.* 29: 461–472, 1926.

of the young died before they were old enough to wean. We had also found that thyroid feeding increased the mortality of the females, since many of them died during or immediately following parturition. In order to study certain phases of the problem we planned an experiment in which the amounts of thyroid fed would be much larger than in the previous experiments.

Two litters of albino rats were selected for the experiment. Each of the litters at the time of birth had been reduced to two males and four females. When they were twenty-one days old they were weaned and divided into two groups, one male and two females from each litter were kept as controls and the other male and two females in each litter were fed thyroid gland. The controls averaged 45.6 grams and the thyroid-fed rats averaged 47.6 grams at the time of weaning when this experiment began.

The diet fed to the rats was a modification of the diet used by Dr. J. R. Slonaker. It consisted of:

	Grams
whole wheat flour	65.0
casein (commercial)	15.0
powdered whole milk	10.0
sodium chloride	1.0
sodium carbonate	1.5
screened alfalfa meal	3.0
Lilly's cod-liver oil	10.0

The dry ingredients were mixed and then thoroughly stirred with the cod-liver oil and finally ground through a meat chopper or worked through a wire strainer.

In addition to fresh water and the diet described above, all the rats were given each day a small piece of milk-soaked whole wheat bread on which was placed the desiccated thyroid gland before giving it to a thyroid-fed rat. In order to make sure that the rats ate the thyroid and bread, each rat was placed in an individual cage until the bread or bread and thyroid had been eaten. Each rat was then weighed and returned to its cage.

The amount of thyroid fed according to the body weight is shown in Table 1.

A daily record of the maximum and minimum temperatures was kept and the lowest temperature recorded (electric heaters were used when necessary) was 56° F., and the highest was 83° F., until April 27, 1928, when the temperature in the room rose to 88° F., and the day was cloudy and sultry (high relative humidity). Between four and five P. M. on that day two of the "thyroids" died. One week later, an May 4, when the temperature rose to 94° F., the remaining four thyroid-fed rats died. In addition to these six deaths four other "thyroids" from another experiment died on these two days, one female died on April 27 and two males and one female died on May 4, but not a single control animal died!

TABLE 1

SHOWING DAILY DOSAGE OF DESICCATED THYROID ACCORDING TO BODY WEIGHT

Body weight (grams)	Amount of desiccated thyroid fed (grams)
50 to 100	.025
100 to 125	.050
125 to 150	.075
150 to 175	.100
175 to 200	.125
200 to 225	.150
225 to 250	.175
250 to 275	.200
275 to 300	.225
300 to 325	.250

On April 27 the rats were ninety-one days old and the "thyroids" had received sixty-seven consecutive doses of desiccated thyroid (Armour and Co.). The amount of each dose had varied according to the body weight, see Table 1. On that date the control males weighed 358 and 406 grams and the females averaged 245 grams as compared with the weights of the "thyroid" males of 234 and 270 grams, and the "thyroid" females averaged only 203.5 grams.

The relatively high temperatures on these two days seemed to be the cause of the death of the ten thyroid-fed rats because five of them were observed to be in respiratory distress just before they died. Respiration appeared to be very difficult and when water was sprinkled by hand upon three of these prostrated animals they were temporarily relieved, but they died when the cooling effect of the evaporating water had disappeared. Autopsies showed that the spleen and kidneys particularly were affected. Yellowish-gray spots of various sizes appeared on the ventral surfaces of the kidneys and the ventral surface of the spleen was black in all of the rats. The blackened area extended from the tip of the spleen and covered as much as one third of the ventral surface of these organs, which were hypertrophied, as were the liver, kidneys and suprarenals. With the exception of one animal in which there had been a severe hemorrhage so that the thoracic cavity had filled with blood, the lungs and the digestive tracts of all of the animals appeared to be normal.

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